

EPA COMMENTS

**DRAFT LOWER PASSAIC RIVER STUDY AREA REMEDIAL INVESTIGATION/
FEASIBILITY STUDY
REMEDIAL INVESTIGATION REPORT – SECTION 7 AND MODELING APPENDICES
DATED APRIL 2015 AND JUNE 2015**

<u>No.</u>	<u>General Comments – Section 7</u>	
361	The CPG models presented in the draft RI include model code and input changes that represent significant modifications to the modeling framework. As stated in the 2007 Settlement Agreement and Order of Consent (EPA 2007) these changes require EPA approval. Unless and until EPA approves the modifications, the RI text should not state that the models conform to either the 2007 Settlement Agreement and Order of Consent or Modeling Work Plan. The CPG must revise their current approach to address the issues listed in Attachment 3 , provide EPA the revised code inputs and outputs for their review and revise the RI text to indicate that modifications to the modeling framework are undergoing review by EPA.	
362	In general, the organization of the information presented could be improved. It is unclear why certain details are presented in Appendix K rather than in Section 7 or the model-specific appendices. Appendix K should be eliminated, and the information presented in Appendix K should be presented either in Section 7 of the RI Report or in the appendices specific to the individual models.	
363	In general, the information presented in the figures should be described with greater detail either in the figure legends or in the text that references the figures. For example, Figure 3 in Appendix K has kilometers at the top of each panel and miles at the bottom; water depth in feet and meters on the left and right sides, respectively, of each panel; a color scale representing total suspended solids (TSS); and red lines representing isohaline contours. This is a large amount of information presented in a condensed format, and should be presented with a greater level of documentation. Please revise the report and figures to ensure that an appropriate level of documentation is provided to allow the reader to interpret the information presented in each figure.	
364	Because model results for different surface sediment depths (i.e., both 0-2 cm and 0-15 cm are used as the surface sediment depth), each text discussion and figure presenting surface sediment results should identify the depth interval over which the results were averaged. Please revise the report and figures to ensure that the surface sediment depth is clearly defined wherever surface sediment results are presented.	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Section 7</u>
365	Section 7.1, page 1, first paragraph, fourth sentence, and Appendix K, Section 1, page 1, first paragraph, fourth sentence, Section 3.1.2, page 9, first paragraph, first sentence	As presented, the models, including the changes that have been incorporated into the contaminant fate and transport (CFT) model, have not been approved by EPA, and therefore do not comply with the 2007 Settlement Agreement and Order on Consent. Furthermore, there are deviations from the 2006 Modeling Work Plan (HydroQual 2006a, 2006b) in the models, including the carbon simplification, the lack of a contaminant hindcast, and the number of contaminants modeled. All deviations from the

		2007 Settlement Agreement and the 2006 work plan should be described and justified in the text.
366	Section 7.1, page 1, second paragraph, first sentence, and Appendix K, Section 1, page 1, first bullet	This sentence should be revised to include the Kill van Kull and Arthur Kill since these are included in the domain of the models.
367	Section 7.1, page 2, first paragraph	Please revise the text to state that the regional model was run by EPA and that all of the boundary inputs described were provided by EPA.
368	Section 7.1, page 2, second paragraph, sixth sentence	Please revise the text to note that navigation scour was included empirically in the model based on interpretation of bathymetric data in limited areas along the western side of the Passaic River below RM 1.5. Please add a figure presenting the cells where navigation scour was incorporated or reference Appendix M, Figure 34.
369	Section 7.1, page 2, second paragraph, last sentence, and Appendix K, Section 3.2, page 10, second paragraph, second sentence	Considerable effort was required to calibrate the sediment transport (ST) model, including the incorporation of new mechanisms in the model (bed forms, the fluff layer, and navigation scour); balancing bathymetry change data, water column solid data, and SEDflume data that suggested conflicting parameterizations for model inputs; adjustments to parent bed and deposited bed critical shear stresses, erosion rates, and layering; and modifications to sediment size classes represented in the model and their settling rates. The statement that “these inputs and parameters have been defined to a large extent by data from the LPRSA, with minimal adjustment during calibration” oversimplifies the very extensive effort that was required to calibrate the ST model to the complex and sometimes contradictory site data. Please revise the text to provide a more accurate description of the ST model calibration process.
370	Section 7.1, page 3, second paragraph, eighth and ninth sentences	Dissolved organic carbon (DOC) is not modeled. It is specified as a constant value and that value is not used in the organic carbon (OC), CFT, or bioaccumulation models. Additional demonstrations of the OC model’s ability to reproduce the Sediment Transport – System Wide Eutrophication Model (ST-SWEM) and data have been requested by EPA, particularly longer-term runs and additional detail on spatial and temporal results, which have not been provided. Please refer to Comment Nos.

		517 to 534 for further information on the requested outputs and concerns with the carbon simplification approach. Please provide the additional analyses requested and modify the text to clarify that water column DOC was not modeled or used by the CPG.
371	Section 7.1, page 4, second paragraph, fifth sentence and footnote 2	The CPG approach of adjusting partitioning settings to prevent sorption to algal and DOC does tend to result in an increase in the contaminant mass returning to the sediment with the resuspended particles, which is the desired result. However, the way in which the inputs are specified in the model results in instantaneous desorption of contaminants from the sediment particles to the dissolved phase, with no algal uptake or sorption of dissolved-phase contaminants to DOC. The latter two processes would be expected to occur on time scales shorter than the desorption time scale. The partitioning approach used is not valid and needs to be corrected. Refer to Comment Nos. 557, 562, 563, and 564 for further discussion of the partitioning approach presented in the RI.
372	Section 7.1, page 4, second paragraph, sixth sentence, and Appendix K, Section 5.2, page 20	Section 5.6 of the Modeling Work Plan (HydroQual 2006a) states: <i>“The Lower Passaic River Restoration Project Contaminant Fate and Transport model calibration for HOCs will be based primarily on the ability of the model to reproduce measured concentrations (historical and current) of dioxin/furan congeners and coplanar PCB congeners in water and sediments.”</i> The calibration results presented are limited to one dioxin congener (2,3,7,8-TCDD) and total tetrachlorobiphenyl. While the two chemicals chosen for calibration represent a range of loading histories, with 2,3,7,8-TCDD contamination more closely related to the former Diamond Alkali facility and tetra-PCB more widespread, the range of chemical properties represented is narrow compared to the range of properties for the 29 dioxin, furan, and PCB congeners that are targeted for calibration based on the work plan text. As an example, the table below presents K _{OW} values for these 29 chemicals plus tetra-PCB; as shown by the highlighted values, the two chemicals used for calibration of the CFT model both fall at the low end of the range of K _{OW} values for the chemicals targeted in the work plan. At a minimum, the RI Report must present summary model calibration results for all 29 chemicals prescribed by the Modeling Work Plan.

		Chemical	K _{OW} *	Chemical	K _{OW}
		Tetra-PCB	6.00	1,2,3,7,8-PeCDD	7.37
		3,3',4,4'-Tetra-PCB (BZ#77)	6.36	3,3',4,4',5,5'-Hexa-PCB (BZ#169)	7.42
		3,4,4',5-Tetra-PCB (BZ#81)	6.36	2,3,3',4,4',5,5'-Hepta-PCB (BZ#189)	7.71
		2,3,7,8-TCDF	6.54	1,2,3,7,8,9-HxCDF	7.95
		2,3,7,8-TCDD	6.65	1,2,3,6,7,8-HxCDF	7.95
		2,3,3',4,4'-Penta-PCB (BZ#105)	6.65	2,3,4,6,7,8-HxCDF	7.96
		2,3,4,4',5-Penta-PCB (BZ#114)	6.65	1,2,3,4,7,8-HxCDF	7.96
		2,3',4,4',5-Penta-PCB (BZ#118)	6.74	1,2,3,6,7,8-HxCDD	8.09
		2',3,4,4',5-Penta-PCB (BZ#123)	6.74	1,2,3,7,8,9-HxCDD	8.10
		3,3',4,4',5-Penta-PCB (BZ#126)	6.89	1,2,3,4,7,8-HxCDD	8.12
		2,3,3',4,4',5-Hexa-PCB (BZ#156)	7.18	1,2,3,4,7,8,9-HpCDF	8.67
		2,3,3',4,4',5'-Hexa-PCB (BZ#157)	7.18	1,2,3,4,6,7,8-HpCDF	8.67
		2,3,4,7,8-PeCDF	7.23	1,2,3,4,6,7,8-HpCDD	8.82
		1,2,3,7,8-PeCDF	7.25	OCDF	9.39
		2,3',4,4',5,5'-Hexa-PCB (BZ#167)	7.27	OCDD	9.55
		*Log K _{OW} values from the FFS model (LBG 2014).			
373	Section 7.1, page 5, first paragraph, third and fourth complete sentences and footnote 4	Although it is likely that there is some small amount of OC associated with non-cohesive solids in the LPR, this was not included in the model because data are not available to directly quantify the amount of carbon that might coat a sand particle, and because of the expected limited fraction of contaminants transported with non-cohesive particles. LPR OC data are limited to bulk samples of mixtures of cohesive and non-cohesive particles. While these data can be analyzed to identify trends of decreasing fraction of organic carbon (f _{OC}) with increasing non-cohesive fraction or with increasing bulk density (an indicator of increasing non-cohesive fraction), it is not possible to discern whether the measured f _{OC} is associated with the cohesive sediment mixed in with the larger particles or with the larger non-cohesive particles themselves.			

		<p>In the Housatonic River, sediments were fractionated by size and f_{oc} measured on the subsamples (Weston 2004a). In the portion of the river characterized by coarse sediments, f_{oc} directly measured on non-cohesive particles averaged approximately 0.3%. Scanning electron microscopy analyses of Housatonic River quartz particles showed only blotchy organic films or coatings (Weston 2004b). In the muddier portions of the Housatonic River, the f_{oc} of the larger particles was over 10% in many cases; however, this was attributed to large pieces of organic matter, including sticks and leaves. Sediment profile images (Germano & Associates 2005) of the LPR confirm the presence of macro-organic material, but this material typically has lower particle densities and behaves differently than the large sand particles represented in the model.</p> <p>Table 1 in Carroll et al. (1994) reports the f_{oc} of particles of different sizes in the Hudson River, including f_{oc} values of 6% and more for particles greater than 293 μm. It is unlikely that these were sand particles with OC coatings based on the findings of other studies. Di Toro et al. (1991) summarize data from Prahl (1982), including measurements of f_{oc} for sand-sized particles, which were further segregated based on density. Sand-sized particles with densities greater than 1.9 grams per cubic centimeter (g/cm^3) exhibited f_{oc} values between 0.2% and 0.4%, while the f_{oc} values of less dense sand-sized particles exceeded 10%. It would be inconsistent to model the transport of low-density, high-f_{oc} particles using the non-cohesive transport equations because these lower-density particles would not be transported as bedload in the same way as the sands that are represented in the LPR non-cohesive solids classes.</p> <p>The vast majority of non-cohesive transport in the LPR is bedload rather than suspended load, and adding bedload to the CFT model would require additional model development and necessitate smaller time steps due to the faster settling velocity of non-cohesive particles. Based on a comparison of Figure 7-2c to Figure 7-2b, the CPG predicts that the gross deposition fluxes for non-cohesive solids (Figure 7-2c) are significantly larger than for cohesive solids (Figure 7-2b), but the net deposition fluxes do not show the same trend. The transport of solids</p>
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		<p>through the system indicated by the fluxes between reaches is dominated by cohesive solids.</p> <p>The CPG should consider if other issues such as dramatic changes in composition, or unrealistic partitioning assumptions, may be producing this issue. If transport associated with non-cohesive solids is significant, as stated in the footnote, the CPG should consider how to incorporate partitioning to sands into the model without requiring unacceptably long simulation times, and provide the proposed model changes to EPA for review.</p>
374	Section 7.2.2.1, page 7, first paragraph, fourth sentence	<p>Because the study area boundary is a diagonal line across rectangular grid cells, fluxes need to be summed across cell interfaces in both the X and Y directions below RM 0.5 where the river meets Newark Bay. Although this process is not as simple as summing across the Y direction interfaces above RM 0.5, the area below RM 0.5 should not be neglected in mass balance figures. This area represents approximately 10% of the surface area of the full 17-mile LPRSA and there are a number of important processes occurring between RM 0 and 0.5 (i.e., navigation scour and deposition during storms followed by subsequent erosion). Please present mass balance results for the complete study area from Dundee dam to the boundary with Newark Bay.</p>
375	Section 7.2.2.1, page 8, second paragraph, second sentence	<p>Figure 7-2 does not demonstrate that the transport of non-cohesive sediments is strongly influenced by high flow events. If this is the case, the model would likely compute larger downstream advective fluxes of non-cohesive sediments corresponding to the large gross resuspension fluxes, particularly in the upstream reaches. Please revise the report and add figures to present mass balance results for a high-flow period to support the conclusion that the higher fluxes of non-cohesive sediments reflect “the strong influence of high flow events,” or revise the text to remove this conclusion. Below RM 2 the gross erosion and deposition fluxes are of the same magnitude. Remove the word “particularly” from this sentence.</p>
376	Section 7.2.2.1, page 8, third paragraph, third sentence	<p>Please revise the text and figures to present mass balance results over the full LPRSA domain demonstrating variations between high- and low-flow results.</p>

377	Section 7.2.3.1, page 9, bullets 1 through 5	<p>Generally, the percentages presented agree with the figures; however, a number of the values differ to an extent that cannot be explained simply by rounding. Please verify the following calculations and revise the text as needed:</p> <ul style="list-style-type: none"> - First bullet: per Figure 7-3b, gross tetra-CB erosion = $2.18 + 20.07 + 42.32 + 17.1 = 81.67$ kilograms per year (kg/yr) and net erosion = $2.18 - 1.79 + 20.07 - 18.06 + 42.32 - 41.19 + 17.1 - 18.61 = 2.02$ kg/yr, which results in a gross to net erosion ratio of 40.4 rather than 26. If navigation scour and diffusion are considered part of gross erosion, this results in a gross to net erosion ratio of 27.6 rather than 26. - Second bullet: The region between RM 14 and RM 8 represents 69% of the net sediment source for tetra-CB rather than 66% ($[0 + 20.07 - 18.06 + 0.06] / [0 + 2.18 - 1.79 + 0.04 + 0 + 20.07 - 18.06 + 0.06 + 0 + 42.32 - 41.19 + 0.06 + 0.81 + 17.1 - 18.61 + 0.01] = 0.69$). - Third bullet: The region above RM 14 accounts for 14.3% of the net tetra-CB flux rather than 12% ($[0 + 2.18 - 1.79 + 0.04] / [0 + 2.18 - 1.79 + 0.04 + 0 + 20.07 - 18.06 + 0.06 + 0 + 42.32 - 41.19 + 0.06 + 0.81 + 17.1 - 18.61 + 0.01] = 0.143$). - Fourth bullet: The region below RM 2 captured a mass equivalent to 8.3% of the net 2,3,7,8-TCDD flux rather than 9% ($[0.71 + 10.74 - 12.16 + 0.02] / [0 + 0.56 - 0.34 + 0 + 0 + 23.48 - 20.18 + 0.11 + 0 + 44.62 - 39.4 + 0.18 + 0.71 + 10.74 - 12.16 + 0.02] = -0.083$). - Fifth bullet: The loading from the Upper Passaic River at Dundee Dam represents 76% of the net tetra-CB flux rather than 73% ($2.29 / [0 + 2.18 - 1.79 + 0.04 + 0 + 20.07 - 18.06 + 0.06 + 0 + 42.32 - 41.19 + 0.06 + 0.81 + 17.1 - 18.61 + 0.01] = 0.76$). <p>Please verify that the numbers presented in the text are consistent with Figure 7-3 and clarify whether navigation scour and diffusion are considered part of gross erosion.</p>
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378	Section 7.2.3.1, page 10, first paragraph after bullets	Please add RM 0 to the discussion in the text and to Figure 7-4.
379	Section 7.2.3.2, pages 11 through 12; Figure 7-6	<p>Appendix O, Figure 4-3 shows a low bias in predicted concentrations between a factor of 2 and 10 for half of the depositional cells. This bias strongly influences the results presented in this section. As an example, for the areas below RM 1 or above RM 7, the 1995 concentrations are based upon approximately 2010 data (Appendix O, Section 3.1.1.5). Therefore, outside of the RM 1-7 reach, the contaminant model results in 2010 should look like the results in 1995, and the model results in 1995 may not appropriately reflect 1995 conditions. Because the entire RM 8-17 reach falls into this category, the last two sets of bars on the bottom panels of Figures 7-5a and 7-5b, as well as the points falling far above or below the 1:1 line on the bottom panels of Figure 7-6, indicate two possibilities: either the 1995 initial conditions for those cells are off by up to four orders of magnitude, or the rate of change is over predicted . If this error exists in the RM 8-17 reach it is likely that impacts results throughout the model. The second paragraph on page 12 states that the model predicts exaggerated recovery in some cells, but does not recognize the importance of that exaggeration with respect to remedy simulation, particularly the simulation of remedies that are dependent on the model's predictive capabilities at grid cell and smaller scales. The over-prediction of recovery in depositional areas is likely the result of an under-prediction of contaminant erosion and transport from other areas. This discussion needs to be revised once the model is corrected to address underestimation of concentrations in depositional areas.</p> <p>Please label the 1:1 line on Figure 7-6.</p>
380	Section 7.2.3.3, page 12; Figure 7-7.	Please add RM 0 to the discussion in the text and to Figure 7-7.
381	Section 7.2.3.3, pages 12 through 13	The discussion in this section highlights the significant impact that the fluff layer has on the CFT model results. The CFT model fluff layer is not based on the ST model

		<p>fluff layer, and is used as a tuning parameter to match predicted water column concentrations with the CWCM data without any constraint on the vertical variation in concentration between the bottom layer of the water column and the 15-cm average sediment concentration. There are water column data and 15 cm sediment data, but only very limited data that were collected at shallower intervals than 15 cm. The eight thinly sliced cores collected during the 2008 LRC program do not show a consistent relationship between the near surface slice and 15-cm average. There are no data to indicate the relationship between the concentration in the fluff layer and the concentration in the top 1 to 2 cm of the sediment. The CFT fluff layer model predictions are then presented as a demonstration of the system response under high flow conditions without any way to verify that response with data. Figure 7-8 should present water column results for the surface and bottom layer in addition to the fluff layer and bed results. Concerns with the CFT fluff layer implementation are discussed further in Comment Nos. 405 and 538 through 544. Model behavior under high flow conditions should be revisited after concerns with the CPG parameterization of partitioning and the fluff layer are addressed.</p>
382	Section 7.2.3.3, page 13, third paragraph, second sentence	<p>The text discusses the water column response to Hurricane Irene, but the water column model results are not presented. Please add the water column predicted particulate concentrations to Figure 7-8 and present predicted total water column results on a volumetric basis for the same period.</p>
383	Section 7.2.4.1, page 14, second paragraph, first sentence	<p>Figures 7-9 and 7-10 underestimate the role of sediments within the food web due to issues with the bioaccumulation model calibration. As shown in the review below in Comment No. 614, the assumption that benthic organism biomass is dominated by deposit feeders is flawed due to non-site-specific organism weights used. This has implications for predator feeding preferences as they were ostensibly based on benthic organism biomasses. After the model is recalibrated, these figures will need to be reproduced.</p>

384	Section 7.2.4.1, page 14, third paragraph, last sentence	Because the two groups of fish will respond differently to remediation, it is important that their relative abundance is properly considered within the bioaccumulation model. Based on abundance data, filter-feeding fish are much less prevalent in the LPRSA than small forage fish, but this is not reflected in feeding preferences of higher predators. As directed below in Comment No. 585 , feeding preferences must be reconfigured in the bioaccumulation model to properly reflect small fish abundance data.
<u>No.</u>	<u>General Comments – Appendix K</u>	
385	The “Alternative OC model” is referred to as the “OC model” in Appendix K, but in Appendix N the model is referred to as the “AOC model.” Please revise the RI Report and appendices as necessary to ensure that the term “OC model” is used consistently and remove all use of the term “AOC model.”	
No.	Page No.	Specific Comments – Appendix K
386	Figures 13, 14, 15, 17, 18 and 19	The combination of CPG models appear to predict a more rapid “natural recovery” in strongly depositional areas than is suggested by the data presented in Figures 13, 14, 15, 17, 18 and 19. The underlying factors that contribute to this miscalibration need to be investigated and corrected prior to use as a management tool.
387	Section 2.2.1, page 3, first paragraph, first sentence (continued from page 2)	Please revise the text to state that the grid is mostly four grid cells across in the lower miles, with a maximum of six across for a short stretch in the Harrison Reach.
388	Section 3.1.1, page 8, second paragraph, third and fourth sentences	Please revise the text to include a statement regarding the limited amount of data above 2,000 cfs.
389	Section 3.1.1, page 9, second paragraph, second sentence, Section 3.3.1, page 12, first	The reference to Hurricane Irene as a “100-year event” is inconsistent with the RI Report, Section 7.1 (page 3, first paragraph, second complete sentence), which refers to Hurricane Irene as a “1-in-90 year storm event.” Characterization of Hurricane Irene as a 90-year event is consistent with the USGS statistics for the storm (refer to

	paragraph, first sentence	http://nj.usgs.gov/hazards/flood/flood1108/ and http://nj.usgs.gov/hazards/flood/flood1108/docs/gagepeaksummaryaug27-30.pdf Page 2, Station 01389500, Passaic River at Little Falls, Recurrence Interval = 90). Please revise the text to be consistent with Section 7.1 of the RI Report and with USGS statistics.
390	Section 3.1.1, page 9, second paragraph, third sentence	Please revise the text to clarify that the erosion near the mouth of the Passaic River during low-flow periods is due to a combination of navigation scour and tidal resuspension. Refer to Comment No. 368 concerning the description of navigation scour in RI Section 7.1.
391	Section 3.1.2, page 9, first paragraph, second sentence	Please revise the text to reflect that although the CPG RI and EPA FFS models were initially identical, both sets of code have been modified. It is true that many of the modifications made to either version of the model have been incorporated into both (e.g., the Sanford consolidation model, the fluff layer representation), however the current versions of the EPA FFS and CPG models are not identical.
392	Section 3.2.2, page 11, first paragraph, fifth sentence	Please clarify the way in which the critical shear stresses were defined for the silts and clays. In addition, this paragraph should be broken into multiple paragraphs for ease of reading.
393	Section 3.2.2, page 11, last sentence, and page 12, first sentence	Please revise the text to clarify how the grain size and dry density data were interpolated onto the grid.
394	Section 3.3.1, page 13, first paragraph, second and third sentences	Please clarify if the calibration of the settling velocities was revised after the calibration of critical shear stresses for erosion from the parent bed or if the calibrations were truly “mutually exclusive” as stated in the text.
395	Section 3.3.2.3, page 14	Please revise the text to acknowledge that at RM 1.4, the model predicts that net upstream transport will continue at higher flows than the data indicate, and that at RM 6.7, less upstream transport is calculated as compared to the data.

396	Section 3.3.2.4, page 15	In the parenthetical phrase that refers to 50% of the 2008-2010 erosion reflecting sub-grid scale localized scour, clarify if the comment applies to erosion in each reach or only in the RM 14-2 reach. Also clarify if the term “and resulting deposition” refers to deposition in the RM 2-0 reach due to localized scour from the RM 14-2 reach. The text overstates agreement between the model and data. The fate of the additional erosion volume from RM 2-14 over 2007-2012 period should be discussed. In addition, the text should summarize results on a gross and net basis for the total area plus the erosional and depositional areas individually. Finally, this section should be broken into multiple paragraphs for readability.
397	Section 4.2, page 17, second paragraph, first and second sentences	Refer to Comment Nos. 532 and 533 related to the f_{oc} bulk density relationship. A power function may provide a better fit of the f_{oc} data than the linear regression that was used. In addition, the text in this section states that the bulk density presented is dry bulk density but that is not stated in Appendix N where this relationship is presented again. In addition to the linear regression presented in the text, alternative regression forms should be tested for fitting the f_{oc} dry bulk density relationship.
398	Section 4.2, page 17, third paragraph	It is unclear why DOC is mentioned in the OC model description. Based on the approach used by the CPG the water column DOC in the CPG’s models is a constant value in both time and space, and is not modeled or used in any of its OC, CFT, or bioaccumulation calculations. EPA did not derive a spatially and temporally constant water column DOC value as suggested by the text, although this was done for the sediment. The CPG must represent water column DOC partitioning in the CFT model and its impacts on bioavailability in their bioaccumulation model. The CPG should perform an analysis of the New Jersey Harbor Dischargers Group DOC data to determine if seasonal or spatial trends exist in the data and represent those trends in the CFT model input. The CPGs RI text will need to be revised once these corrections are made to the model.
399	Section 4.3, pages 17 through 18	The comparison of the OC model results to ST-SWEM results was not done using the OC model as implemented in the RI. As noted in Comment No. 534 , longer test

		simulations should be conducted using the model as implemented in the RI and compared to the available water column chlorophyll- <i>a</i> and particulate organic carbon (POC) data.
400	Figures 9a through 13b	Please revise these figures to plot all contaminant concentration panels (volumetric or solids-normalized) using consistent scales on the y-axis. Using different scales tends to mask the spatial similarity in the observed data across stations, events, depths and flow conditions. In addition, please print the model median and data median concentrations on the plots, and include a label for the x-axis of the panels.
401	Figures 15a and 15b	The CFT model is biased slightly high with respect to the 2010 TCDD concentrations in the erosional areas and biased low with respect to the 2010 tetra-CB concentrations in the erosional areas, but the model is biased low for both TCDD and tetra-CB in the strongly depositional areas. This might result in an overestimation of the rate of recovery. Please correct the model to address this bias and revise the text accordingly.
402	Section 5.2, pages 20 through 23	Please revise the text regarding the model application to the LPR based on the discussion of these topics in Comment Nos. 535 through 565 on Appendix O.
403	Section 5.3.1, page 23, third sentence	The text implies that it is inappropriate to test the model's performance based on spatially and temporally paired values for the water column and spatially paired values for the sediment. Please clarify why these paired values are used in the comparison of model output and data distributions. Refer to the description of pairing in Appendix O, Section 4.1.1. Given the small footprint of the CPG's previously proposed remedial alternative, it is important that the model appropriately represent both the temporal and spatial behavior of contaminant transport within the LPR.
404	Section 5.3.1, page 23, fifth sentence	If it is "beyond the capability of the model to resolve sub-grid scale phenomena," then the model cannot be used in the FS to assess the benefits of remedial alternatives applied at scales finer than the grid cells.

405	Section 5.3.1, page 23, last sentence (continued on page 24)	The application of the fluff layer particle mixing must be corrected (refer to Appendix O, Comment No. 542) and the value constrained to be greater than or equal to the Layer 1-2 mixing. This would be consistent with the vertical structure of mixing the CPG has proposed as well as their proposed feeding structure dominated by organisms residing at the surface of the sediment and feeding from the fluff layer. The fluff layer thickness and its transfer to the bed should be consistent with the ST model. Please correct these inconsistencies.
406	Section 5.3.2.2, page 25, first sentence	Please clarify the statement that the model provides “a reasonable vertical concentration profile.” There are no vertical data profiles available against which to compare the model; the computed vertical profile simply fits the assumptions made by the CPG modeling team.
407	Section 5.3.2.2, page 27, second paragraph, sixth and seventh sentences	While assessing the model to data comparison on a cohesive solids basis does provide some improvement in model skill, the model is still biased low in depositional areas both on a cohesive solids basis and an OC-normalized basis. Expand the analysis to separate the depositional results into the mildly and strongly depositional categories presented in Figure 15.
408	Section 6.2.1, page 31, second paragraph after first set of bullets, first sentence	Model calibration focused on model performance while averaging all data over the modeling areas as a single zero-dimensional (0D) segment is not appropriate, as discussed in Comment Nos. 574, 577, 579, 594, 597, 600, and 601 below. There are significant differences by RM in sediment concentrations, fish tissue concentrations, salinity, oxygen, and organic matter content, among other factors. The model performance must be examined at a finer spatial scale by creating model to data comparisons by RM bins.
409	Section 6.3.2, page 33, first sentence	By focusing the model calibration on model performance for six target species, the CPG has eliminated a large set of data from their model performance testing, especially forage fish (n=4), mummichog (n=18), perch (n=22), and benthic invertebrates (n=19). EPA directs the CPG to include these species in the model performance testing.

		The calibrated model performance is reasonable based on the limited metrics presented. However, the breadth of metrics presented is not wide enough to demonstrate the model's performance. As directed below in Comment No. 609 , the CPG will need to recalibrate the bioaccumulation model.
<u>No.</u>	<u>General Comments – Appendix L</u>	
410	Figures should be described in sufficient detail within the text for the reader to easily understand the content of each figure, and sufficient information should be provided on each figure for the reader to easily comprehend all the information being presented (e.g., Section 2.2.1 and Figure 2-10). In addition, units displayed in legends and on figures should be consistent throughout (e.g., no units on Figure 2-3 and 2-4 and meters on Figure 2-5). Please revise the text and figures to ensure that the information presented is clear and consistent. All time series plots should be illustrated consistently (e.g., colors used for model and data on Figure 2-35 and 2-38), and line widths selected so that modeled and measured data can be easily identified (e.g., Figure 2-21).	
411	<p>Please revise the text to avoid the use of vague statements such as: “These references were not eliminated in the restart files, but <u>other things were changed within this file</u>” (Section 2.2.6, page 17, first paragraph, last sentence), “The format for the input files of the heat flux conditions was changes from the calibration runs” (Section 2.2.5) “changes were made to the format of gcmts, gcmplt and the restart files” (Section 2.2.6) and “Post-processing tools needed to be modified to meet the new requirements” (Section 2.2.6).</p> <p>The text should either include details about what was changed, if relevant, or state that the changes were not relevant.</p>	
412	Figures presented in landscape orientation (e.g. Figures 6-1 – 6.5) should have captions in landscape orientation.	
413	Section 2.1.2, page 12, third paragraph, fifth and sixth sentences, Section 2.2.1, page 16, fourth and fifth sentences	Please renumber and reorder the figures as necessary or revise the text so that the figures are sequential in the text and figure sheets. In this section, Figures 2-14 and 2-15 are called out before Figures 2-9 through 2-13, which are not referenced until later sections. Figure 2-11 is called out before Figure 2-10.
414	Section 2.1.7, page 15	Please revise the text to identify where the Tierra Solutions, Inc. (TSI) instruments were located during

		deployment and where the shipboard transect measurements were completed. References to the corresponding reports should be included.
415	Section 2.1.8, page 15	Please revise the text to identify where the Rutgers instruments were located between 2000 and 2002. References to the corresponding reports should be included.
416	Section 2.1.9, page 15	Please revise the text to identify where the Rutgers instruments were located in 2004. References to the corresponding reports should be included.
417	Section 2.2, page 15, second paragraph, first sentence	Please revise the text to clarify whether the referenced modifications to the ECOM code are those made by HDR, Inc. (HDR) before the code was passed along to the CPG, or if additional modifications were made by the CPG.
418	Section 2.2, page 16, first paragraph, fourth complete sentence	Please add the missing word "removed" between the words "was" and "entirely."
419	Section 2.2.1, page 16, last sentence	Please clarify the statement that "The changes were designed to "straighten" the Hackensack as the turning of the model was no longer required to increase computational time."
420	Section 2.2.5, page 17	Please augment this section with additional information regarding the changes from the calibration runs.
421	Section 2.2.6, page 17, second paragraph, fourth sentence	Please clarify the purpose of the statement that "There is no clear description of which variables are time dependent, and which are purely initial conditions for the hot started model." It is unclear whether this description is needed, or if the intent is to establish a record of what was/was not provided to the CPG.
422	Section 2.2.6, page 17	The discussion in this section suggests that there were changes made to the model code and input files that were not adequately explained to the CPG or given to the CPG. Please clarify whether this is the case and, if so, whether this information has been requested by the CPG.

423	Section 2.3, pages 18 through 20	Please revise this section to include discussion of the model performance metrics. Quantitative statistical comparisons should be computed on the model-data performance to provide a metric other than qualitative description of agreement.
424	Section 2.3, pages 18 through 20	Some of the figures referenced in this section include data that appear suspect (e.g., spikes in velocity on the top panel of Figure 2-22). Please revise this section to indicate whether there was an attempt to filter suspect data and if so, provide a description of the process by which unreliable data were vetted and filtered and the criteria applied to determine when and when not to use these data.
425	Section 2.3, page 19, last paragraph, third sentence	Please revise the text to clarify whether “the dry case” refers to the very low flow case.
426	Section 2.3, page 20, fourth paragraph, fourth sentence	Please revise the text to include more description to fully define the information plotted on all subplots in Figure 2-38. For instance, U*S should be defined in the text and on the figure.
427	Figure 2-3, page 24 and Figure 2-4, page 25	Text on page 12 describes Figures 2-3 and 2-4 as showing bathymetry used in the model, however there appear to be some blank (white) cells in the figure. Please modify the figures to present the bathymetry for these cells. Also, please explain how data were treated if coverage of a grid cell was less than 100%. In addition, please add units to the legends.
428	Figure 2-10, page 30	Please clarify what this figure is illustrating, including identifying which panel shows the calibration grid and which shows the 10-year run grid and labeling the axes.
429	Figure 2-14, page 34	It appears that there might be a plotting error in the figure as the coloring appears in a zig-zag pattern up the river channel. Please verify and re-plot the figure if necessary.
430	Figure 2-20, page 39	Please clarify whether the discharge data were obtained from the Little Falls gage or somewhere else.

431	Figure 2-26, page 44	It appears that there might be a bias in the way the salinity data in these figures (2-25 2-26, 2-27 and 2-35) were measured that prevented salinity from being measured at 0 ppt. If a bias is identified in the measured data and the data in the figures are shifted accordingly (vertically), the measured data may better align with the modeled salinity.
432	Figure 2-38, page 54	It is unclear exactly what is being plotted in this figure. Please provide additional description in the text and/or the figure to clarify.
433	Section 3.2.3, page 58, third sentence	Please clarify whether the “EPA survey from 2005” is the survey completed by Tierra Solutions, Inc. in 2005. A reference should be added to the text.
434	Figure 3-1, page 60	The red dots shown in the figure should be included in the legend. This figure should be replaced with a larger, clearer version.
435	Figure 3-2, page 61, bottom subplot	Please revise this figure to plot the wind direction as points rather than a line to avoid connecting directions on either side of the 360/0-degree threshold.
436	Section 4.0, page 67, third paragraph, third sentence	Please verify the date of the “large discharge event” referenced in this sentence. It appears that the year should be 1996, not 2006.
437	Figures 4-2 and Figure 4-3, page 69	Please label the x-axes of these figures. It is unclear whether the numbers are miles or kilometers.
438	Figures 6-1 through 6-5, pages 79 through 83	Please clarify whether any quantitative model performance metrics were computed to assess model-data agreement. Some quantitative measure of performance should be estimated and presented in these figures to show model agreement performance.
439	Section 6.4, page 94, second paragraph, last sentence, Figure 6-14	Please clarify whether reference to “event #1” in the text refers to the panel on Figure 6-14 labeled “routine event #1”. In addition, text should be included to characterize the agreement of the data and model results for high-flow event #1, which are shown on Figure 6-14, but not discussed in the text. The panel labeled “High-flow Event

		1" shows that the model tended to under-predict the measured salinity concentrations for the upper end of the range of the salinity data.
440	Tables A-1 and A-2, page 99	Please verify whether the correlation coefficients, all greater than 0.98, provided in the water level and discharge statistical comparisons are correct given the tabulated root mean square (RMS) errors. In addition, please verify that the correlation coefficients (r) in the tables are not the coefficient of determination (r ²).
441	Appendix B, page 109, second sentence	This sentence is incomplete. Rather than omit these flow data, the amount of rainfall at the time should be reviewed to determine whether the flow of 35,000 cfs is consistent with the rainfall data and should be included in the flow frequency analysis. Please revise the text and frequency analysis accordingly. Alternatively the analysis should be presented with and without this value included.
<u>No.</u>	<u>General Comments – Appendix M</u>	
442	The discussion of the comparisons between model results and data focuses on instances where model results are in better agreement with data and does not provide a balanced treatment of the cases where the comparisons are not as favorable. Please revise the text to include balanced discussions of the model-data performance.	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix M</u>
443	Section 1.0, page 12, second paragraph, last sentence	Please note the typographical error and revise "and presented" to read "are presented."
444	Section 2.3.3, page 17, last paragraph, third sentence	Please correct the subject-verb agreement in this sentence by revising "were normalized" to read "was normalized."
445	Section 2.3.10, page 24, last two sentences	Please revise the text to present these two sentences as a hypothesis rather than a fact. Considerable uncertainty is often evident when attempting to assess bed evolution by comparison of bathymetric surveys. While navigation scour is a possible explanation for survey differences, it has not been proven to be the cause.

446	Section 4.2.4, page 38, last paragraph, sixth sentence and Figure 20	The text states that "Square and circle symbols are used to denote the rating curve derived estimates." Figure 20 also includes triangular symbols. Please revise the text accordingly and include the triangular symbols in the legend in Figure 20.
447	Section 4.2.4, page 40, last paragraph, and Figure 23, page 105	Please add a notation to Figure 23 to explain what the red lines represent and include this explanation in the discussion of Figure 23 in the text.
448	Section 4.2.5, page 47, first paragraph, fourth sentence	Please revise the text to clarify whether the negative entrainment rates were excluded from the development of the functional relationship discussed in the text.
449	Section 4.2.5, page 50, last paragraph, fourth sentence	Please revise the text to provide justification for using a cutoff of >50% cohesive solids in the data screening, given that a criterion of <15% cohesive content (as reported in Section 4.3.2, page 55, second bullet) is used to distinguish non-cohesive from cohesive behavior of the active layer.
450	Section 4.3.2, page 55, third bullet	The reasonableness of using the initial D50 to calculate skin friction needs to be demonstrated by showing limited temporal variation in D50 through long-term simulations or by conducting sensitivity analyses with alternate D50 values, since the model results indicate that significant variations in D50 evolved during the calibration simulation. The text will need to be revised to provide justification for use of the initial D50 once such analysis has been performed.
451	Section 4.3.4, page 56, last paragraph, fifth through seventh sentences	The effect (or lack thereof) of the lag should be demonstrated by repeating the calibration simulation without the lag. The text should then be revised to discuss the results of this comparison. It is recognized that this will increase the overall runtime for the demonstration simulation.
452	Section 5.4.3, page 64, first paragraph, third sentence	Please correct the reference to "Attachment XXX" with the appropriate attachment.
453	Section 5.4.3, page 64, second	Please correct the subject-verb agreement for "salt front...are" by either changing "are" to "is" or removing the parentheses around "and the ETM."

	paragraph, second sentence	
454	Section 5.4.3, page 66, first paragraph, last sentence	Please correct the reference to “Appending [sic] XXX” with the appropriate attachment and revise “Appending” to “Attachment.”
455	Section 5.5, page 73, third paragraph, first sentence	Please revise the text to clarify whether “both cohesive sediments” refers to clays and silts.
456	Section 6.0, page 78, second paragraph, first sentence	Please revise the text to explain why the under-prediction of infilling and preferential infilling of the shoals is attributed to the sigma-coordinate system as opposed to other factors, such as critical shear stresses, as stated in the second numbered item in this sentence.
457	Figure 25	The physical water column monitoring (PWCM) data collected prior to April 10, 2010, which the text indicates were excluded in the development of the rating curve (Section 4.2.4, page 42, first bullet, third and fourth sentences), should be identified on Figure 25 using different symbology.
<u>No.</u>	<u>General Comments – Appendix M, Attachment B</u>	
458	Throughout the attachment, the unit system of measurement is inconsistent. Measurements are sometimes reported in English units, sometimes in SI units, and sometimes in both. Please revise the attachment to ensure consistency in the system of measurement used.	
459	Please revise the text to ensure that each figure is fully explained so that it is clear to the reader the point that is being made with each figure, and that figure legends identify all symbols and lines on the associated figure. For example, in Figure 2-3, the inset panels taken from Dr. Chant’s work are not explained and would not be clear to many readers, and there should be a legend to clarify what the different colored lines indicate.	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix M, Attachment B</u>
460	Section 2.0, pages 11 through 14	The figure references within the text for Figures 2-2 through 2-6 include the figure number and title. Please revise the text to only include the figure number when referring to each figure.

461	Section 2.0, page 11, last paragraph, fifth and sixth sentences (continued on page 12), and page 12, Figure 2-3	Please revise the text to provide a more detailed explanation of Figure 2-3, including the graphic overlays of Dr. Chant's data (with a reference to the report from which these overlays were taken). In addition, please revise the figure caption to be grammatically correct by adding the word "in" between "Computed variation" and "salinity intrusion," and add a legend to identify the blue and green lines.
462	Section 2.0, page 13, Figure 2-6	Please revise the text to explain the figure in greater detail since there is a lot of information being conveyed (e.g., Little Falls and Dundee Dam flow; tidal range; likely scour events; etc.). Please include an explanation of the rectangle outlined in red on the left side of the figure. Please revise the note stating "no events between 2007 & 2008" to clarify that this is referring to the 2007 and 2008 multibeam surveys, since there was an erosion/scour event in early 2007. In addition, please update this figure since the 2011-2012 multibeam bathymetry survey data are available and can be included. Please remove the second part of the figure title ("from Fig. 2-1 it can be expected..."). Instead, since the 2011-2012 data can be evaluated, include a statement in the text that there were no major scour events that occurred between the 2011 and 2012 surveys.
463	Section 2.0, page 14, second paragraph, second sentence	Please revise the text to clarify how Figure 2-5 suggests that "elevated TSS values are to be expected at river flows beyond $Q_{riv} = 6000-7000$ cfs."
464	Section 2.0, page 14, second paragraph, third sentence	Please revise the text to clarify the statement that "Also from other observations, a similar threshold was found." The text should include specific mention of these "other observations" and where they are shown.
465	Section 3.3., page 18, second paragraph, fourth sentence	Please revise the text to clarify what is meant by "significant" sedimentation by providing a quantitative measure. In addition, please note the typographical error and correct the spelling of "sedimentation" (currently written as "sedimention").
466	Section 4.2, page 22, Table 4-2	Please change the criterion for mean bias in the performance testing over a soft bottom, with depths

		between 15 and 40 feet, to ± 0.2 feet since the mean bias may be positive or negative.
467	Section 4.4, page 31, Figure 4-10	Please add a legend to the figure to clarify what the horizontal lines represent.
468	Section 4.4, page 31, Figure 4-10	Please define the acronym LWR in the figure caption.
469	Section 4.5, page 33, Figure 4-11	The figure legend is difficult to read. Please revise the figure to make the legend more legible and explain which colors denote erosion and deposition in the figure.
470	Section 4.5, page 34, Figure 4-12	Please revise the figure to correct the cropping of the right side of the legend labels. In addition, please clarify the meaning of the yellow coloring, which is labeled as "< threshold (.1 ft)," while the figure title states that "Bed level changes smaller than 0.5 ft are ignored as within accuracy." In addition, please move the latter portion of the figure caption, beginning with "Bed level changes...", to the description of Figure 4-12 in the text. Finally, please change the colors of the lines indicating RMs to be different from the colors used in the legend, since these lines have nothing to do with the erosion/deposition patterns at the associated RM.
471	Section 4.5, pages 40 through 41, Figures 4-17 and 4-18	Figures 4-17 and 4-18 are not referenced or described within the text. Please revise the text to ensure that all figures are discussed.
472	Section 4.5, page 42, first sentence (continued from page 41)	Please revise the text to clarify the statement that "apparently, the flux towards the turbidity maximum is more important than the volume of sediment in the turbidity maximum itself."
473	Section 4.5, page 44, last bullet	This bullet point seems out of place as the sentence above the bullet points implies that the discussion is regarding the source of fine sediments that fill scour holes, while this bullet point describes coarse sediments. Please revise the text to clarify how this point relates to the discussion of fine sediments. In addition, please correct the misspelling of "coarser" as "courser."

474	Section 4.6, page 44, second paragraph, first sentence	<p>USACE (2010) provides the approximate drafts of vessels that transit certain areas of the LPR. Table 5 in the USACE report, which is based on a 1997-2006 study (nearly 10 years old), indicates that drafts up to 33 and 34 feet are observed, but these drafts represent less than 1% and 5%, respectively, of all trips recorded for the corresponding berths. In addition, these vessels remained near the mouth of the river, traveling no farther than RM 0.6.</p> <p>It is also unclear how these deeper draft vessels would be able to transit any portion of the LPR at the present time, as a review of the 2012 bathymetry indicates that -26 feet National Geodetic Vertical Datum (NGVD) is the (approximate) deepest bed elevation in the navigation channel, which is equivalent to -24.5 mean low water (MLW) in that area. Vessels with drafts deeper than this would require a very high tide in order to transit the river without impacting the sediment bed directly. Therefore, their frequency of transit up the river is likely to be relatively low. Based on these observations, it is reasonable to assume that, while vessels may have some impact on keeping the navigation channel in a sort of "depth equilibrium," the channel might also remain in equilibrium mostly as a function of hydrodynamics. Please revise the text to provide a more balanced discussion of the potential causes of this observed equilibrium in the channel.</p>
475	Section 4.6, page 44, second paragraph, third sentence, and page 45, Figure 4-21	<p>Please revise the text to describe the procedure used to compute the water column velocities resulting from tug boat propellers, including the equation(s) used and appropriate references. In addition, please clarify whether the "distance from the propeller" referenced in the caption of Figure 4-21 is computed as vertical distance (i.e., depth below propeller), horizontal distance, or slant distance from the propeller.</p>
476	Section 4.6, page 45, second sentence, and page 46, Figure 4-22	<p>Automatic Identification System (AIS) data should be retrieved for the same time period as the bathymetry survey data (i.e. 2011-2012) in order to provide a more representative comparison than the March-June 2013 AIS data. Please revise the text and figure accordingly once these data have been retrieved.</p>

477	Section 4.6, page 45, fifth sentence, and page 46, Figure 4-22	It is not clear from the figure that “the ship track and frequency maps show good correspondence with the areas of observed scour.” The bathymetric difference panel illustrates erosion over nearly the entire area of study, so is it difficult to discern where the increased ship traffic density has caused specific patterns of erosion or scour. The ships are required to remain within the navigation channel to navigate safely, and the bathymetric change in the channel indicates erosion over most of the area of study, but, spatially, that appears to be the extent of the correlation in the data. Please revise the text to clarify how the data presented in the figure demonstrate correlation between ship traffic and observed scour.
478	Section 4.6, page 46, Figure 4-22	Please revise the bathymetric difference panel to clarify that blue coloring indicates erosion and red coloring indicates deposition.
479	Section 4.6, page 46, Figure 4-22	The vessel density data show where the more frequent ship tracks were during this time period, but there is no information provided about vessel speed or draft, which would have an even greater impact on the resuspension of sediments. A risk rating system should be created that is a function of ship draft, speed, and track, which would better illustrate the spatial risk of resuspension. Please revise the text and figure to reflect this analysis.
480	Section 4.6, page 46, second paragraph, and page 47, Figure 4-23	The text and figure imply that there is a large mass of eroded sediment that is available to the water column between RM 0 and RM 1.4 during low-flow periods in the LPR. However, it is unclear how much of this sediment is estimated to be transported upstream of RM 1.4 and how much is expected to be transported downstream to the Newark Bay Study Area (NBSA). Please revise the text and figure to clarify.
481	Section 4.6, page 49, third sentence	Not all of the vessel-scoured sediment in the lower miles of the LPR will be transported upstream to the LPRSA. Please revise the text to clarify how the resuspended sediment is thought to be distributed in terms of upstream vs. downstream transport.

482	Section 4.6, page 49, second sentence	Equilibrium bathymetry is also likely a strong function of the hydrodynamics of the LPR at a given point in time. While vessel traffic and types of vessels will have a stronger short-term impact on resuspended sediment, the hydrodynamics and tidal fluctuations of the LPR are likely to have a stronger long-term, continuous impact. Please revise the text to consider the relative impacts of short-term vs. long-term dynamics when evaluating sediment bed dynamics.
483	Section 5.3, page 53, fourth sentence	It is true that the lowest contaminant concentrations were observed in areas with little to no sedimentation. However, some of the highest concentrations were also observed in such areas (e.g., Figure 5-4, 2008-2009 Data; Figure 5-5, 1995 Data; Figure 5-6, 2008-2009 Data). Please revise the text to clarify that the contaminant concentrations in areas with low sedimentation rates span up to four orders of magnitude in range.
484	Section 5.4, page 55, Figure 5-7	Please revise this figure by adding the 1:1 line to allow easier visualization of how the cesium-based and bathymetry-based sedimentation rates compare.
485	Appendix B, page 61, first paragraph, second sentence	Please revise the text to clarify when and why the 1-foot by 1-foot or 5-foot by 5-foot resolution data were used when evaluating the correction needed for the 2008 bathymetry. It is unclear why both resolutions are referenced within this appendix. Please make this clarification throughout Appendix B as needed.
<u>No.</u>	<u>General Comments – Appendix M, Attachment C</u>	
486	Although the entrainment rate analysis may provide a useful screening-level constraint on the shear stress-dependent erosion rate of the fluff layer, the limitations of the analysis should be presented more thoroughly in the report. In particular, the analysis is based on a very simplified approach to ST in the reach of RM 1.4 to RM 4.2 (Figure 2). Although the report makes an initial point of distinction between direct measurement of erosion rate (e.g., via SEDflume) and analysis of “entrainment rate” from differences in suspended sediment concentration (SSC) across the RM 1.4 to RM 4.2 reach, the ultimate application of the entrainment rate analysis is an erosion rate formulation for the fluff layer (Equation 2) that functions exactly the same as formulations of erosion rate from SEDflume measurements. However, unlike the <i>measured</i> shear stress in a SEDflume, the shear stress used in Equation 2 in this analysis is a spatial average	

	<p>of <i>modeled</i> shear stress for grid cells along the thalweg within the RM 1.4 to RM 4.2 reach. Even if one presumes that the model accurately simulates bed shear stress over the entire reach, the spatial averaging suggests that Equation 2 would tend to underestimate the fluff layer erosion rate for higher-than-average shear stress and overestimate it for lower-than-average shear stress, which could throw off the spatiotemporal response of the fluff layer dynamics.</p> <p>As a “validation” step, it would be instructive to perform the entrainment rate analysis over a different time interval to assess the magnitude of variation in the results. Suitable supporting data are likely limited, so this validation might also be performed for arbitrary subsets of the existing time intervals. Please revise the text to discuss the results once this validation has been performed.</p>	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix M, Attachment C</u>
487	Section C.1, page 2, first paragraph, third sentence	Please clarify the meaning of this sentence by deleting the extraneous word “and” from the phrase “the fluff layer and overlying less erodible strata.”
488	Section C.2, page 4, first paragraph, second complete sentence	Please revise the end of the sentence to read “RM 1.4-4.2.”
489	Section C.5, page 7, first paragraph, fifth sentence	Please revise the text to provide a quantitative measure of the proportion of the dataset affected by the variability in SSCs in lieu of the phrase “a relatively small subset of the entire dataset mainly at the lower velocities.” Based on Figure 3, it appears that negative entrainment rates represent between approximately 15% and 20% of the dataset. Near-bottom velocities for these negative entrainment rates vary from approximately 0.3 to 0.7 meters per second (m/s), which encompasses a similar range to that of the positive entrainment rates, rather than being limited to the lower velocities.
490	Section C.6, page 8, Figure 4	Please revise the figure to clarify if the black symbols represent the binned shear stress-entrainment rate pairs and whether negative entrainment rates were omitted before binning.
<u>No.</u>	<u>General Comments – Appendix M, Attachment D</u>	
491	This attachment compares the parameterization of fluff layer erosion developed using PWCM data to independent erodibility data collected by Chesapeake	

	<p>Biogeochemical Associates (CBA, 2006) using Gust microcosms. In general, the comparison is reasonable, given the uncertainties associated with both techniques. The critical stress chosen for the fluff layer parameterization, 0.25 dynes per square centimeter (dynes/cm²), is about 40% lower than the most frequent estimate of surface critical stress from the microcosm tests, 0.4 dynes/cm², and the fluff layer critical stress remains constant while the microcosm critical stresses increase rapidly with depth. However, the reasonable agreement between the fluff layer erosion rate constant (both linear and power law forms; Figure 6) and those measured with the microcosm is encouraging. Similar comparisons in the literature often don't agree as well as those presented here. Ultimately, the chosen parameterization results in a greatly simplified but reasonable approximation of the PWCM data, in the same range as the microcosm results.</p> <p>However, the presentation in this attachment overstates the similarities between the fluff layer parameterization and the microcosm data, particularly with respect to the critical stress profile. Furthermore, the derived fluff layer parameterization (Equation 3 and Figure 5) is not unique and should not be presented as such, as alternate combinations of parameter values can provide essentially the same outcome.</p>	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix M, Attachment D</u>
492	Section D.3, page 4, Figure 3	Presentation of the microcosm critical stress profile data on log-log plots artificially accentuates the very near-surface and very low critical stress regions of the curves, making it appear as though the profiles exhibit an extended region of constancy near the surface, followed by a rapid increase to a high critical stress. In fact, the observed rate of critical stress increase with depth begins immediately and varies between a 0.5 to 1 power dependence on depth. In addition, the assumed constant critical stress of the fluff layer parameterization is not shown. Please revise this figure to present the data on linear axes and clearly show the chosen constant value of 0.25 dynes/cm ² on all plots for comparison.
493	Section D.4, page 6, Figure 5	The agreement between PWCM-derived entrainment (erosion) rate and the parameterization of Equation 3 with $A = 1.1 \times 10^{-5} \text{ cm/s}/(\text{dynes/cm}^2)^n$, $\tau_c = 0.25 \text{ dynes/cm}^2$, and $n = 0.75$ is good. However, though the use of a linear ($A=1 \times 10^{-5}$, $\tau_c=0.25 \text{ dynes/cm}^2$, and $n=1$) fit is discussed in the text below the figure, it is not presented in Figure 5 for comparison. The linear fit is the basis of the comparisons to microcosm data in Figure 6 and therefore should be

		presented in Figure 5. While the chosen fluff layer erosion parameterization results in reasonable behavior, it is not necessarily the best possible or the only acceptable parameterization. Please revise the figure to include the linear fit and the corresponding R^2 value. The agreement of the linear fit with the data might be nearly as good as the nonlinear approximation, perhaps even better agreement if the first bin-averaged entrainment rate from the PWCM data is ignored. The main point is that, though the chosen fluff layer erosion parameterization results in reasonable behavior, it is not necessarily the best possible or the only acceptable parameterization.
<u>No.</u>	<u>General Comments – Appendix M, Attachment E</u>	
494	Please clarify the text throughout this attachment to ensure that the parameter averaging techniques are described consistently. To fully understand the procedures used to compute the sediment erosion parameters (e.g. A , n , and El), detailed explanation of when arithmetic and log-averaging techniques were applied should be included. Refer to Comment No. 506 for a specific example.	
495	Please clarify the text throughout this attachment to describe any filtering criteria that were applied to parameters when computing the critical shear stresses. For example, as discussed in Comment No. 499 , please identify any filtering criteria that were applied to the regression coefficients, A and n , prior to averaging.	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix M, Attachment E</u>
496	Section E.2, page 2, first paragraph, first sentence	Please revise the text to change the distance units between duplicate cores from feet to meters, consistent with units reported in Borrowman et al. (2006). As stated in that report, “Replicate cores were therefore anywhere from one to 10 meters apart from one another, which both explains some of the differences in bulk property and erosion rate behavior and illustrates the extent of heterogeneity of Passaic River sediments.”
497	Section E.2, page 2, first paragraph, fourth sentence	Please revise the text to clarify that 6 cores were collected from 3 core <i>locations</i> up-estuary of RM 8, and 22 cores were collected from 11 core <i>locations</i> between RM 0 and RM 8.
498	Section E.3, page 3, first paragraph,	Please revise the text and figures so that the variable d represents the top of each depth interval, rather than the

	fourth sentence, and pages 10 through 37, Figures 3 through 30	midpoint, to indicate the depth range over which each regression is applied. The actual length of each depth interval is unclear when using the midpoint of the depth interval in the figures.
499	Section E.3, page 3, and Figures 3 through 30	Please revise the text in Section E.3 to clarify how the A and n values were filtered when averaging and whether outlier A and n values were used in the averaging (e.g., Figure 3, subplot 6, lower right with $n = 0.20413$). In addition, please clarify whether τ_{Cr} values of zero (e.g., Figure 4, subplot 5, lower middle) or unusually large τ_{Cr} values (e.g., Figure 28, subplot 3, upper right with $\tau_{Cr} = 0.34$ Pascal [Pa]) were used in the averaging.
500	Section E.4, page 4, text following Equation 3	Please revise the text “as shown in Equations 3 and 4” to refer to Equations 4 and 5 instead.
501	Section E.4.1, page 4, first paragraph	Please revise the text to clarify whether the term “depth interval” is synonymous with “shear stress sequence.” If not, please explain the difference between the two terms.
502	Section E.4.1, page 4, first paragraph, fifth sentence, and page 38, Figure 31	Results for depth intervals containing only two data point pairs should not be included in the regression analyses or subsequent averaging because the uncertainty associated with the accuracy of the regression in determining the critical shear stress cannot be quantified without at least three data point pairs. Please revise the text and figure as necessary after removing these data from the analysis.
503	Section E.4.1, page 4, first paragraph, sixth sentence	Please revise the text to clarify whether the phrase “highly variable EI values” refers to comparisons of the EI values for the intervals with only two data pairs to EI values for the intervals above and below those intervals.
504	Section E.4.1, page 4, first paragraph, eighth sentence	Please revise the text to include discussion of those cores that are exceptions to the general trend described in this sentence (e.g., P09B, P07B, P03A, etc.).
505	Section E.4.2, page 5, third sentence	The down-core trend of critical shear stress appears to be variable for many of the cores shown in Figures 32 through 35. Please revise the text to clarify and provide supporting analysis for the general statement that “the

		trend on average is of increasing critical shear stress with depth into the bed.”
506	Section E.4.3, page 5	Please revise the text to clarify the averaging techniques that were applied at each step to the A and n values when computing the EI spatial patterns. For example, please clarify whether the log-averaged A and arithmetically averaged n values from each core were also log-averaged and arithmetically averaged, respectively, across the domain to compute a site-wide average.
507	Section E.4.3, page 5, last sentence	This sentence mentions a “lack of spatial variability in erodibility,” which is inconsistent with preceding descriptions. For example, the sixth sentence in this paragraph notes that “ EI is seen to vary over 2 orders of magnitude, with even the duplicate cores collected at a given location varying by an order of magnitude in some instances.” Please revise the text to reconcile this and similar statements with the last sentence in the paragraph.
508	Section E.6, page 6, first paragraph, fifth sentence	<p>Please revise the text to clarify how the erosion properties were averaged to develop a single set of parameters. For example, please clarify whether log-averaging or arithmetic averaging was used and whether erosion rates and power law regression coefficients were averaged.</p> <p>Based on the language from the last sentence in Section E.1 (“The following sections describe....the resulting development of appropriate model inputs”), it is assumed that the “average core” described in Section E.6 is what is used as (or to develop) model input critical shear stresses. Please revise the text to clarify whether this is the case.</p>
509	Section E.6, page 7, Table 1	The values in Table 1 for the down-core critical shear stresses are lower than those in the CPG’s model input files. Based on the ST model input file “erate.sdf,” the down-core critical shear stresses are approximately 50% larger than the empirical critical shear stresses listed in Table 1. Please add a footnote to the table or revise the text to clarify whether the calculated or empirical critical shear stresses listed in Table 1 were utilized in the ST model and revise the text in Section E.6 to clarify how the model input files were generated.

<u>No.</u>	<u>General Comments – Appendix M, Attachment F</u>	
510	<p>This attachment presents an analysis of water column monitoring data to derive characteristic bulk settling velocities for ST modeling, based on fitting classic Rouse SSC profiles to the observations. The data analysis and, as a result, the derived bulk settling velocities are reasonable, with a few minor caveats as discussed in the specific comments below. However, explanations of the observed behavior are biased toward the ultimate parameterization, while alternate explanations are discussed but ultimately ignored. More importantly, the ultimate parameterization for settling velocity (two particle classes with different settling velocities based on particle source) is presented very suddenly on page 8, in the last paragraph of the document, with very little connection to the preceding discussion. Furthermore, this parameterization is incompletely described and is not compared to the data, as discussed in Comment No. 516. While it may be a reasonable representation of settling behavior for modeling purposes, the presentation in the text needs to be expanded. Please revise this attachment to provide a more balanced and complete discussion of the parameterization selected as well as alternate possibilities.</p>	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix M, Attachment F</u>
511	Section F.2, page 3, Equation 2	<p>Overall, Sections F.1 and F.2 are well written and convincing, and the method proposed to derive empirical settling velocities is reasonable. However, it is unclear whether the turbulent Prandtl-Schmidt number (σ_T) of 0.7 used in Equation 2 is also used for the turbulent diffusivity of suspended sediment in the ultimate numerical model. Using $\sigma_T = 0.7$ in the data analysis and then implicitly setting $\sigma_T = 1$ in the numerical model (by ignoring it) would be equivalent to overestimating settling velocities by $1/0.7$. Please revise the text to clarify whether the Prandtl-Schmidt number of 0.7 used in the analysis was carried through to the model.</p>
512	Section F.3, page 4, paragraph after bullets, second sentence	<p>Excluding data from the bottom-most and surface-most bins of the acoustic Doppler current profiler (ADCP) is reasonable, since they may be biased by blanking distance issues and spurious reflections, respectively. The explanation provided in the text, that they are excluded due to “suspected turbulence,” should be expanded to specifically describe the type and source of the suspected turbulence. Please revise the text accordingly.</p>

513	Section F.3, page 5, fourth paragraph	<p>It is reasonable to dismiss flocculation as an explanation for the larger settling velocities during flood than ebb at RM 1.4. However, attributing this behavior only to mobilization of larger particles or transport of larger particles from downstream during flood is too restrictive. In fact, the second to last paragraph on this page implicitly points out the most likely reason for the observed behavior when it states that water column stratification causes the Rouse approach to fail (i.e., to overestimate settling velocities). It is apparent based on the salinity stratification during flood tide, shown in Figure 5, that the water column is frequently more stratified during flood at RM 1.4, while estimated settling velocities during flood are also larger. This is acknowledged in the discussion of Figures 12 through 17, but it is not acknowledged as a likely reason for the larger settling velocities observed during flood at RM 1.4. Please revise the text to include this explanation, even if it does not immediately support the final parameterization chosen.</p>
514	Section F.4, page 7, fourth paragraph	<p>The paragraph discussing potential relationships between settling velocity and SSC is overly dismissive. While it is true that the data shown in Figures 21 through 23 indicate a significant amount of scatter, a general pattern of higher settling velocities at higher SSCs is apparent. Figure 23 in particular shows a clear relationship between settling velocity and SSC at higher SSCs at RM 13.5. More importantly, while this analysis shows clear relationships between near-bottom water velocity and bulk particle settling velocity, it is unclear if or how these relationships are used in the model. If higher settling velocities at higher flow velocities are associated with resuspension of larger particles, as stated in the text, there should be an associated correlation between SSC and settling velocity, as more total particles should also be resuspended at higher flow velocities. However, as currently described in this attachment, the settling velocity parameterization does not explicitly incorporate either of these factors. Please expand the discussion in the text to more completely address the relationships among near-bottom velocity, mass and size of resuspended solids, and settling velocity.</p>

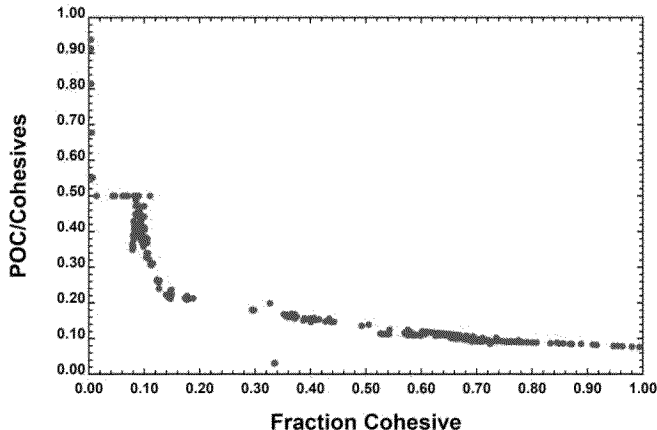
515	Section F.4, page 7, last paragraph	Figure 25 is a good summary figure showing an upstream decrease in median settling velocity. However, the explanations for this trend presented at the bottom of page 7, while plausible, are not the only possible explanations and are not clearly related to most of the previous discussion. Please revise the text and provide additional figures to more explicitly demonstrate the connections between these potential explanations for the upstream decrease in settling velocity and the previously described behaviors. This might include, for example, a graph depicting an upstream decrease in tidal velocity that accompanies the observed upstream decrease in median settling velocity.
516	Section F.4, page 8	The parameterization of the observed behavior as the sum of the behaviors of two independent particle classes of different origin and different settling velocities is abruptly proposed at the end of Section F.4. While this may be a reasonable way to parameterize sediment settling behavior in the LPR, it is not necessarily the only possible explanation and its relationship to the preceding analysis needs to be more explicitly described. Please revise the text to clarify how this parameterization helps to explain the previous observations of relationships between water velocity and settling velocity at different locations; how these two particle classes will be implemented and what their characteristic settling velocities are; whether there will be any exchange between the two classes; and how this parameterization compares to the previously presented data.
<u>No.</u>	<u>General Comments – Appendix N</u>	
517	The text in the introduction states that ST-SWEM was eliminated from the CPG modeling approach in part due to schedule concerns. The ST-SWEM carbon model simulation speed is as fast as or faster than the CFT model and considerably faster than the ST model. The carbon model could have been implemented without modification with little or no impact to the overall schedule. In addition, the CPG has stated that they have concerns with the ST-SWEM model calibration, and EPA agrees that modifying the calibration to address those concerns would likely impact the schedule. Given the CPG's concerns, the CPG should present the OC model (referred to as the Alternative Organic Carbon [AOC] approach) results compared to site data. Please revise this appendix to present these results as implemented in the RI/FS rather than	

	modified to match ST-SWEM, for a long-term (1995-2013) simulation, on a spatial and temporal basis. The results presented here, which were modified to match ST-SWEM for a year, do not reflect how the model was applied to the RI/FS.
518	The CPG's OC modeling approach does not conserve the mass of OC in the sediment. For the modeled contaminants (2,3,7,8-TCDD and tetra-PCB), nearly the entire contaminant mass in the sediment is particle-bound. Because contaminant resuspension is tied to the computed solids resuspension, virtually the same contaminant mass would resuspend regardless of the POC concentration. While this may not be important in terms of resuspension of contaminants, it may affect the partitioning in the water column and the flux of contaminants back to the sediment. If the CPG wishes to use their current approach, a sensitivity analysis should be performed to demonstrate that the lack of a POC mass balance around the sediment does not adversely affect CFT model contaminant predictions in the sediment and water column. Please revise this appendix to include discussion of the results once this analysis has been performed.
519	The presentation of information in this appendix is not as clear as other portions of the RI Report. It reads more like an outline than a report in some sections (e.g., Section 2) and includes grammatical errors (e.g., "A subsequent sections will address the importance of this on the contaminant simulation results." on page 10). Please review the entire appendix and make corrections as necessary. In addition please replace references to "water phase" with "water column" to be consistent with the other sections of the RI.
520	The period of 1 year used to compare the two approaches to modeling OC is too short. The difference between the bed concentrations in the two models at the end of 1 year is approximately 13%. As stated in the second paragraph on page 10, the AOC approach shows an approximately 6% increase in POC in the active layer at RM 2.0, while the ST-SWEM model shows an approximately 7% decrease. This occurs despite the ST-SWEM model having a higher concentration of algal carbon throughout most of the year, in particular June through September, when the ST-SWEM algal carbon concentrations are generally several times greater than the AOC concentrations (Figure 2). Part of the difference in bed POC might be explained by the fact that the AOC model uses a constant settling rate for phytoplankton, while ST-SWEM uses a temperature-dependent rate tied to the fact that the viscosity of water, which affects settling rate, is a function of temperature (ST-SWEM uses an Arrhenius temperature coefficient or θ of 1.027). The CPG should consider implementing a temperature-dependent settling rate in the AOC approach as well. However, the primary concern is whether the POC concentrations in the two models continue to diverge over time and by how much, which should be demonstrated with

	decadal-scale simulations. Please revise this appendix to present the results once these simulations have been performed.	
521	The discussion in Section 4.2 concerns whether to assume time-variable or temporally constant OC dynamics in the sediment bed. The decision to simplify the OC model to eliminate the explicit computation of phytoplankton growth and the multiple detrital carbon classes in the sediment bed would still require the computation of time-variable OC dynamics in the sediment bed. Please revise the text to provide justification for the assumption of a constant f_{OC} in the bed, along with results for a multi-year run performed using both approaches to evaluate the impact of a constant f_{OC} on the CFT model results.	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix N</u>
522	Section 1.0, page 5, third paragraph, first two sentences	Although the OC simplification proposed by the CPG was accepted in concept, there are some outstanding concerns (refer to Comment Nos. 517 through 534). These concerns must be addressed before the RI can state that there is a consensus on the CPG's approach.
523	Section 2.2, page 6, fourth bullet	Please remove this bullet from the text if the resuspended POC is based entirely on assigning a fixed f_{OC} to the bed, as stated in Section 4.2. If the approach is modified such that the bed POC is computed as a mass balance assuming an 85% loss of newly deposited algal carbon and no loss of the conservative detrital carbon, no edit is needed.
524	Section 2.2, page 7, second bullet, and Section 2.3, third bullet	In the test results presented in Section 3.0, a mass balance was computed for the bed POC. However, the mass balance approach is not used when the CPG applies the model in the RI/FS, and therefore the test is not directly applicable to the CPG's modeling approach. The initial approach for simplifying the OC model which the CPG described in conversations with EPA and their consultants would have used a fixed ratio to the total solids in the bed; however, as described in this document, the bed POC was set as a fixed ratio to the cohesive solids rather than the sum of the non-cohesive and cohesive solids. The description, testing, and application of the OC model should use the same approach to handling bed carbon. These bullets need to be expanded to clarify how the data were used to define the carbon concentration of the resuspended solids.

525	Section 2.3, page 7, first bullet	Please revise this bullet to include the resuspension of sediment POC as a source of water column POC.
526	Section 3.0, pages 8 through 18	Please replace or supplement Section 3.0 with a demonstration of the model's ability to reproduce carbon data, particularly water column POC, as well as a demonstration of the impact of the lack of a carbon mass balance.
527	Section 3.2, page 9, first sentence	Please remove the reference to grid cell 323, as the reference is unclear and the RM and grid indices are sufficient to identify the location. Results for additional locations should also be presented with the CPG's OC model compared to data.
528	Section 4.1, page 19, second paragraph	<p>The approach described in the text would maintain the mass and average bulk density for the two layers (the active layer equal to the top 15 cm and the archive layer equal to the remainder of the bed). The bulk density of both layers would therefore be overestimated at the surface and underestimated at the bottom. This error would be small for a single point in time, but may present issues due to the time variable nature of the bulk density computed by the ST model.</p> <p>If the bed OC is computed based on a mass balance, the constant thickness of the active layer will result in continuous numerical mixing of POC between the two layers each time there is deposition and subsequent erosion. If the bed f_{OC} is kept constant, this numerical mixing will not be an issue. The CPG will have to provide additional diagnostic information before EPA can approve the use of the CPG's current, constant f_{OC}, approach (refer to Comment No. 521).</p> <p>Based on the text it is not clear how the CPG has handled consolidation, a concern previously noted by EPA. For example, if a large amount of cohesive sediment is deposited following a storm, and consolidation then occurs without any additional deposition or erosion, both the active and archive layers will increase in bulk density and decrease in volume. Assuming 30 cm of deposition, 15 cm would initially be added to both the active and archive layers. Following consolidation without any additional erosion or deposition, the thicknesses would be 15 cm and</p>

		<p>8.3 cm in the active and archive layers, respectively, using the CPG's ST model parameterization. It is unclear how the CPG's version of the OC model handles this bed elevation change. Given the fixed volume structure of the CFT model bed layers below the top layer, it is also unclear how this change in bulk density is handled when it is passed forward from the OC model to the CFT model. Finally, it is unclear what errors would result if the thickness change is applied at the sediment-water interface, rather than distributed over depth, and weighted toward the bottom of the deposit. The resulting volume change should not be applied only to the top layer or the very bottom layer in either the OC or CFT model. As previously stated, this is not a concern in the OC model if the ratio of POC to cohesive solids is constant and a mass balance is not achieved. The primary concern is in the CFT model, where the archive stack is not set up to handle changes in layer thickness over time.</p> <p>Please revise the text to address how consolidation was handled in the revised code to ensure that the transfer of solids, OC and contaminants between the CFT model layers is represented consistently. If the contaminant mass in a layer is conserved, but the bulk density of that layer is adjusted without changing the volume, the result is a change in dry weight concentration, which should not occur.</p>
529	Section 4.2, page 20	The chosen approach does not conserve the mass of POC in the sediment. Please revise this section to address the impact of the chosen approach, particularly the flux of carbon to the water column and the resulting impact on the flux of carbon to the sediment on the computed contaminant results.
530	Section 4.3, page 20, first paragraph, second sentence	Please revise the text to clarify whether the three EPA carbon pools (inert, labile, and refractory carbon) have been appropriately summed into the single carbon pool represented in the OC simplification.
531	Section 4.3, page 20, second paragraph, third sentence	A maximum value of 50% carbon on cohesive sediments is at the high end of the range for pure organic matter. In addition, application of the minimum 5,000 milligrams per liter (mg/L) POC results in a ratio of POC to cohesive solids as high as 94%. The figure below (created by HDR using

		<p>CPG's model outputs) presents this ratio plotted against the sediment fraction cohesive using output from the CPG's CFT model for time zero of the long-term calibration for 2,3,7,8-TCDD for the grid cells within the study area. Please adjust the maximum value for carbon on cohesive sediments and the minimum value for sediment POC so that it does not result in f_{oc} values greater than the stated maximum.</p> 
532	Section 4.3, page 20, second paragraph, and page 21, Figure 16	<p>Based on Figure 16, the data suggest that a power function would fit the observed relationship between f_{oc} and bulk density better than a linear regression. Please reevaluate the data presented in Figure 16 and discussed in Section 4.3 to determine whether an alternative analysis would provide a better representation of the relationship between f_{oc} and bulk density.</p>
533	Section 4.3, page 22, Figure 18	<p>Please revise this figure after reevaluating the data presented in Figure 16 using a power function, as requested in Comment No. 532, in order to obtain a better comparison between the WY9596 initial condition and the observed data in the lower portion of the LPR (RM 0 to RM 7).</p>
534	Section 5.0, page 23, third paragraph, sixth sentence	<p>Although the differences between the two approaches had only a minor effect on the model results, the model has not been implemented as tested, and therefore the actual differences in the results may be larger than presented in Appendix N. As noted in Comment No. 520, the testing should also be conducted for a longer period to verify model performance. Please revise this appendix to present OC model results compared to water column POC</p>

		and chlorophyll- <i>a</i> data and ST-SWEM, using a longer-term simulation (1995-2013) and running the model as it was implemented in the RI/FS. Prior to completing this comparison, please address concerns over the lack of a carbon mass balance (Comment Nos. 518, 523, 524, and 526) and how consolidation will impact both OC and CFT model results (Comment No. 528).
<u>No.</u>	<u>General Comments – Appendix O</u>	
535	<p>Technical arguments can be made to support the modifications to the CFT model related to the fluff layer, vertical mixing parameterization, and non-equilibrium partitioning, but together these modifications tend to minimize the quantities of contaminants that are near the surface layer of the bed and that can be reintroduced into the water column. This appears to result in a more rapid “natural recovery” in strongly depositional areas, which is not supported by the data (see Figures 15a and 15b in Appendix K; in the bottom panel, labeled “Strongly Depositional,” the concentration decreases in time and underpredicts the data in 2010 due to the rate of decline between 1995 and 2010). This discrepancy is also evident in comparing the solids-normalized model results to the data, as shown in Figures 4-3a and 4-3b (in the middle panel, labeled “Depositional,” the distribution of model results falls well below the distribution of the data). The CPG hypothesizes, perhaps in an effort to correct this discrepancy that contaminants may be sorbing to non-cohesive solids. The justification for this hypothesis, discussed in Comment No. 562, is presented in part in Section 4.2, in the second paragraph on page 38, with reference to Figures 4-4a and 4-4b. However, it is not clear from these figures that the model is biased low in areas with a fine/cohesive sediment fraction less than 20%. For 2,3,7,8-TCDD, there are only two data points between RM 1 and RM 7 with which to make a comparison. Between RM 0 and RM 17, where there are more data, the model appears to bracket the two very low data points (less than 5% fine sediments and a concentration of approximately 1 ng/kg) and the three higher data points (between 5% and 15% fine sediments and concentrations between 10 and 20 ng/kg). Furthermore, the model appears to be biased low for both 2,3,7,8-TCDD and tetra-CB in areas with a fine/cohesive sediment fraction between 20% and approximately 50%. This may be due to the combined effects of the fluff layer, vertical mixing parameterization, and non-equilibrium partitioning, which would also affect areas with lower OC content. This bias must be corrected and the text updated to reflect the corrections before the model can be used for application to the RI/FS.</p>	
536	<p>The use of the term “parent bed” in this appendix is inconsistent with its use in the ST model. In the ST model, the parent bed refers to sediment in place at time zero of the simulation with any deposition on top going into depositional layers.</p>	

	In Appendix O, “parent bed” refers to any bedded sediment at the surface of the active layer. Please revise the text to use a different term for the surficial bedded sediments in the CFT model. Note that the comments on Appendix O use the term “parent bed” for consistency with this document.	
537	Results of each of the model sensitivity analyses discussed in the RI Report should be presented in figures and tables for comparison to the chosen set of calibration parameters. It is not necessary to present every iteration of the model, but where it is stated that the result had a significant impact on the model calibration or resulted in significant deviations from the previous model application, the results should be presented in the RI Report (e.g., computed water column and sediment concentrations should be presented for the model before and after the addition of the fluff layer and partitioning changes).	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix O</u>
538	Section 2.1.1, page 3, bullets	The example presented in this section suggests that the solids in the surface layer (top 0.5 to 2 cm) of the sediment bed have a contaminant concentration 10 times higher than the water column solids. The water column contaminant data and sediment contaminant data from the top 0.5 feet of the bed generally do not support a gradient of that magnitude. The contaminant concentrations at the surface of the sediment are more likely to fall in between 100 and 1,000 ng/kg, resulting in a lower ratio. If the concentrations in the surface layer were as high as 1,000 ng/kg, the solids in the water column would likely reflect higher concentrations. Please revise this section to present model results for a longer-duration run (1995-2013) with and without the fluff layer incorporated to demonstrate how the fluff layer impacts the model results.
539	Section 2.1.1.1, page 4, fifth and sixth bullets and footnote 1	This combination of assumptions will eliminate diffusive exchange between the parent bed and the water column when there is a fluff layer present. Because the fluff layer thickness transfer to the parent bed is an exponential decay term, the only time the fluff layer will not be present is under continuously erosional conditions. Furthermore, the model code includes a minimum fluff layer thickness (presently set as 1 μm , or half the thickness of one clay particle), ensuring that there is always a fluff layer present even under continuously erosional conditions. It is not valid to assume that tidal

		<p>resuspension is much greater than diffusive exchange at all times and in all locations. Although diffusive exchange is not a significant factor for the contaminants tested in the reach-scale mass balance presented in RI Report Figure 7-3, it may be important for other COPCs that must be addressed by the RI/FS, or on a more localized basis. Diffusive exchange between the adjacent layers of water column, fluff layer, and parent bed or water column and parent bed must be represented in the model. Please revise this section accordingly once the necessary changes to the model have been made.</p>
540	<p>Section 2.1.1.1, page 4, seventh bullet</p>	<p>The fluff layer cannot have the same bulk density as the surface layer of the parent bed since, as stated in the first paragraph in Section 2.1.1.2, the fluff layer is “a thin surface layer of <i>unconsolidated</i> sediment.” Please make the necessary corrections to the model to represent the fluff layer using the properties computed by the ST model and revise the text accordingly.</p>
541	<p>Section 2.1.1.2, page 5, Equations 1 and 2</p>	<p>Although the CPG has shut off partitioning to DOC in the water column, the model appropriately represents partitioning to DOC in the sediment bed. If the partitioning behavior in the fluff layer matches the parent bed, then these two equations should include DOC partitioning. Please correct these equations and the subsequent equations that depend on Equations 1 and 2 (Equations 3 through 17).</p>
542	<p>Section 2.1.1.2, page 6, Equation 3</p>	<p>The equation should read</p> $\frac{dC_{fluff}}{dt} = \frac{Q_{in}C_{in} - Q_{out}C_{out}}{V_{fluff}} + \lambda_{fluff}(C_{parent} - C_{fluff})$ <p>for the fluff layer and</p> $\frac{dC_{parent}}{dt} = \frac{Q_{in}C_{in} - Q_{out}C_{out}}{V_{parent}} + \lambda_{parent}(C_{parent} - C_{fluff})$ <p>for the surface layer of the sediment, or, if written in terms of total volume,</p> $\frac{dC_{total}}{dt} = \frac{Q_{in}C_{in} - Q_{out}C_{out}}{V_{total}} + \lambda_{total}(C_{parent} - C_{fluff})$ <p>However, if the equation is written in terms of total volume, as currently presented in the text, then the equation creates mass transfer when there is no gradient in concentration. Assuming there is no concentration gradient,</p>

		$\left(\frac{H_F}{H_P} \right) = \left(\frac{H_F}{H_P} \right)$ <p>and assuming that the parent layer is thicker than the fluff layer,</p> $H_F > H_P$ <p>then the equation as presented results in the following:</p> $\left(\frac{H_F}{H_P} \right) > \left(\frac{H_F}{H_P} \right)$ <p>Please revise the text to correct the errors in this equation and the dependent equations (Equations 4 through 17).</p>
543	Section 2.1.1.2, pages 5 through 7	<p>Equations 1 through 17 as presented are incorrect due to the errors in Equations 1, 2, and 3 noted in Comment Nos. 541 and 542 that were carried through the derivation of Equations 4 through 17. Fortunately the application in the code describing mass transfer between the fluff layer and parent bed is nearly correct. The only issue with the application of the derived equations in the code is that λ^2 should be replaced with $\lambda \times H_F$. The error in the model code results in slower mixing between the parent bed and fluff layer. Assuming the rate of particle mixing between the fluff layer and parent bed should be at least as large as the rate of particle mixing between the top two layers of the parent bed, the particle mixing rate is underestimated by a factor of 3 when H_F is at its maximum value (0.001 meter [m]) and H_P is at its minimum value (0.005 m), and by a factor of 10,000 when H_F is at its minimum value (0.000001 m) and H_P is at its maximum value (0.02 m). Please correct this error in the code and revise the text accordingly.</p> <p>Note that the semi-analytical solution approach presented may not be necessary for particle mixing if the minimum fluff layer thickness is increased slightly. If revised as discussed in the previous paragraph, this semi-analytical approach could also be used to represent diffusive mixing between the fluff layer and water column. The derivation can be greatly simplified by first computing the dissolved contaminant concentrations and then solving the diffusion equations.</p>
544	Section 2.1.1.2.2, page 8, bullets	The porosity and OC content of the fluff layer are defined by the assumption that the fluff layer has the same

		properties as the surface layer of the parent bed (as stated in Section 2.1.1.1, page 4, seventh bullet). However, if the fluff layer porosity is not equal to the parent bed porosity, it should be greater than, not less than, the parent bed porosity. Similarly, the f_{OC} in the fluff layer should be greater than that of the bedded sediments if the fluff layer includes solids entering from external sources where the more labile fraction has not yet decayed. That is, the water column f_{OC} should be greater than the fluff layer f_{OC} , which should in turn be greater than the parent bed f_{OC} . Please revise the text to eliminate this section once the derivation of the fluff layer mixing equations is corrected in the model.
545	Section 2.1.1.2.4, page 9, Equation 18	Please clarify whether the value of k_f used in the CFT model is the same as in the ST model. If not, please correct this inconsistency.
546	Section 2.1.3, page 10, last sentence (continued on page 11)	Please revise the text to clarify whether the removal of bed layers due to the bed elevation change applied at the beginning of each time chunk is equal to the change computed during that same time chunk in the ST model, or if there is a lag of one or more time chunks. In addition, please clarify whether the solids, POC, and contaminant loads are distributed uniformly over depth in the ST, OC, and CFT models, respectively.
547	Section 2.1.4, page 11, fourth bullet	The model should not be used to predict changes at sub-grid scales, and unrealistically large reductions in concentration should not be associated with dredging small fractions of a grid cell. For grid cells that were capped at RM 10.9, the areal fractions addressed and corresponding concentration reductions should be presented in the RI Report. The same information should also be presented for the grid cells remediated as part of the alternatives presented in the FS Report.
548	Section 3, page 13, first paragraph, second sentence	Please revise the text based on the discussion of chemicals targeted for calibration in Comment No. 372 .
549	Section 3.1.1.2, page 14, third	The section of the navigation channel included in the harbor deepening project should be considered separately from the other portions of the channel. Please revise the

	paragraph, first sentence	model and the text accordingly, and incorporate additional sampling data as it becomes available (e.g., Newark Bay Phase III sediment sampling data).
550	Section 3.1.1.3.1, page 16, last paragraph, item 2c and footnote 6	Please revise the text to provide an objective basis for choosing 30 ng/kg TCDD in the bottom segment of each core as the threshold for deep zeroing. This assumption eliminates the measured peak in 12% of the cores. The discussion in footnote 6 should also provide additional detail about the sensitivity analysis and its outputs.
551	Section 3.1.1.3.1, pages 16 through 17, and Section 3.1.1.4, page 18	Please refer to on Appendix J. Please revise the text to provide a strategy to ensure that zeroed values are not spread laterally during interpolation to areas that should show contamination in the same depth interval of the sediment bed.
552	Section 3.1.1.3.2, page 17	If areas of shallow sediment overlying hard bottom exist at a scale that is relevant to the associated grid cells and will not result in zeros interpolated to neighboring grid cells that do not have hard bottom, those areas should be identified and treated accordingly across all models. Areas of coarse material that may contain a smaller fraction of cohesive solids should not be treated the same as actual hard bottom. An initial check of the CPG ST and CFT models indicates that during the calibration period, no erosion into zeroed areas occurs, with the exception of two grid cells that erode more than 5.5 feet in the late 1990s. Although these cells are well upstream, near the Saddle River (grid indices I = 17 and J = 234, 236), and have low contaminant concentrations, they do represent a solids load that would dilute concentrations elsewhere in the domain. Please correct this instability in the ST model and adjust the bed properties for non-erodible locations consistently in all models. See Attachment 4, Figures 1a and 1b .
553	Section 3.1.1.5, page 18, and Attachment 1, Figures 13 through 22	The text states that 2010 mapping was used for 1995 initial conditions outside the RM 1 to RM 7 reach. Model inputs received from the CPG in December 2014 indicate that there were a number of grid cells outside the RM 1 to RM 7 reach where sediment initial concentrations for 1995 were not equal to sediment initial concentrations for 2010 (See Attachment 4, Figure 2). Please expand the

		spatial extent shown on Attachment 1, Figures 13 through 22, to include the full 17-mile LPRSA and clarify the statement in Section 3.1.1.5 that contaminant initial conditions for locations outside of the RM 1 to RM 7 reach are identical for the 1995 and 2010 mapping.
554	Section 3.1.1.7, page 19	Please revise the text to clarify why incomplete cores were not included in the calculations. The interpolated values in the locations and depths associated with those cores should be compared to the data that were excluded. In addition, in cases where a concentration of zero was assumed at the bottom of a core, please clarify whether the concentration profile above that point suggests that this is a reasonable assumption.
555	Section 3.1.2, page 21, items 2 and 3 and Figures 3-8c and 3-8e	The text states that a long-term calibration run was performed to establish a surface mean-normalized bed shape (Figure 3-8c), which was then “applied to the initial sediment concentration to establish a vertical structure for the top 15 cm by multiplying the shape with the value from the surface mapping for the initial conditions of interest.” Please revise the text to clarify how the shape shown in Figure 3-8c, which has a near-surface mean normalized 2,3,7,8-TCDD value of approximately 0.45, and the 0-15 cm average concentration for 2,3,7,8-TCDD of 3.58e-4 mg/L (printed on Figure 3-8d) resulted in the near-surface concentration of approximately 1.0e-4 mg/L shown in Figure 3-8d (rather than 1.61e-4 mg/L).
556	Section 3.1.2	Attachment 4, Figure 1 shows the vertical profile of initial conditions for a number of grid cells of interest. The first two cells (Figures 1a and 1b) discussed previously in Comment No. 552 have excessive erosion; the next two cells (Figures 1c and 1d) show locations where contaminant concentrations were zeroed at depth; and the final two cells (Figures 1e and 1f) show extremely low concentration discontinuities in the profile of 2,3,7,8-TCDD concentrations. It appears that there is an issue in the vertical interpolation approach presented in this section of the report. Please correct the error that generates these discontinuity errors, revise the text to reflect those corrections, and provide figures displaying the vertical profile used in the model along with the data used to generate the profiles for the locations with

		discontinuities, locations with erosion in excess of 15 cm, and locations zeroed at depth.
557	Section 3.4, page 25, last paragraph (continued on page 26)	While the cited references argue for reducing the transfer of contaminants from resuspended sediments, none suggest that eliminating that transfer is appropriate. The site-specific high-volume (HV) CWCM field data include measurements of both the dissolved and particulate fractions, which reflect the degree of equilibrium for these two phases while the measured solids are suspended in the water column. The phases other than the detrital POC-bound carbon (freely dissolved, DOC, and algal POC-bound) would have longer residence times in the water column and are likely to be much closer to equilibrium with each other. Please revise the model and the corresponding text to represent partitioning to each of these four phases in a way that is consistent across models and appropriately reflects the total dissolved and total particulate HV-CWCM data, the bioavailable freely dissolved fraction, and the algal POC-bound fraction. Please refer to Attachment 6.
558	Section 3.5, page 30, second paragraph, last sentence (continued on page 31)	A purely non-cohesive fluff layer should not exist. Please correct this by using the ST model fluff layer results and revise the text accordingly.
559	Section 3.6, page 31, second paragraph, first sentence	Please revise the text to clarify how all of the particle mixing processes noted in the previous paragraph are represented in the model if the depth of sediment mixing represented in the model is "due to bioturbation alone." In addition, please clarify what datasets were used to determine the distribution of benthic biomass over depth.
560	Section 3.6, page 32, last paragraph	As noted in Appendix K (Section 5.3.2.2, page 26, second paragraph, last three sentences), the calibrated mixing profile was not applied uniformly due to an error that affected approximately 12% of the LPR cells. Please revise Section 3.6 to be consistent with the description in Appendix K. The CPG CFT model simulation for the combined long- and short-term calibration periods takes under a week to complete. Given this short time required

		to correct the error, model results should be corrected rather than presenting results for a model with known errors in the RI Report.
561	Section 4.1.1, page 34, first paragraph, second sentence	<p>The description in the text suggests that the data were matched in space by grid cell, and that 20% of the data and model results were then discarded and the remaining ranked results compared for the eight remaining 10-percentile bins within a given RM. Please clarify the description if this is not the case. Otherwise, please revise the text to present the justification for discarding 20% of the data. In addition, this does not appear to be an appropriately rigorous quantitative test for a model that is being proposed for use in assessing a remedy at a resolution finer than individual grid cells and targeting the highest 25% of the data across the entire study area (approximately 15% of the area) and the highest 19% of the data across the RM 1 to RM 7 reach (approximately 15% of the area). Please revise the model and the corresponding text to include quantitative calibration metrics that provide insight into the model performance at scales relevant to the remedial alternatives that will be proposed in the FS.</p>
562	Section 4.2, page 38, second paragraph, first sentence, and Figures 4-4a and 4-4b	<p>An analysis of the CPG's hypothesis, that the lack of contaminant partitioning to non-cohesive solids results in an underprediction of concentrations in sandy, depositional areas, was explored considering the top panel of Figure 4-4a. Because initial condition data are not available for the rest of the 17-mile LPRSA (upstream of RM 7 and downstream of RM 1), those locations were not considered in the analysis performed for this review.</p> <p>Attachment 4, Figure 3 presents the CPG's figure reproduced from the model outputs that the CPG provided to EPA in December 2014. The figure presents the 2,3,7,8-TCDD concentrations in the top 15 cm of the sediment plotted against the model cohesive fraction for all cells between RM 1 and RM 7 with deposition greater than 0 cm. The red points on this figure generally reproduce the model results presented on the top panel of Figure 4-4a, with some unexplained exceptions.</p> <p>Attachment 4, Figure 3 is repeated in Attachment 4, Figure 4 with model results for three points in time: the CPG initial conditions, the end of the long-term</p>

		<p>calibration, and the start of the short-term calibration. For all three time horizons, all of the model grid cells between RM 1 and RM 7 with predicted deposition of 0 cm or greater between 1995 and 2010 are plotted. Note that the blue diamonds representing the model initial conditions do not include any values for cohesive fraction less than 35%, and the model does not show the relationship between cohesive fraction and concentration observed in the data because the blue diamonds do not extend into the lower fraction cohesive range. After the long-term calibration period, the model, represented by the red squares, develops a number of non-cohesive cells and a relationship between contaminant concentration and fraction cohesive. Finally, the initial condition for the short-term calibration and projections, represented by the green triangles, shows no relationship between cohesive fraction and concentration.</p> <p>Additional concerns arise upon taking a closer look at Figure 4-4a. If the same figure is generated for the cells that have a fraction cohesive of less than 40% at the end of the long-term calibration, all of those cells started with significantly higher cohesive and 2,3,7,8-TCDD concentrations. After 15 years of simulation, the model reproduces the general shape of the data in the top panel of Figure 4-4a. In addition, the model contaminant concentrations show no clear relationship with fraction cohesive above a fraction cohesive of 60%, which represents more than 60% of the depositional grid cells. This suggests that underprediction of contaminant concentrations in cells experiencing non-cohesive deposition could only be an issue in not more than 40% of the grid cells; however, this underprediction issue extends to a larger fraction of the CFDs presented in Figure 4-3. Please verify that the predicted change in composition is supported by the data and that the data presented are in depositional areas based on the bathymetry data and not the model.</p> <p>The short-term model calibration initial conditions should reflect the observed relationship between cohesive fraction and contaminant concentration shown in Figures 4-4a and 4-4b. By replacing the lower concentrations associated with non-cohesive sediments at the end of the long-term calibration (Attachment 4, Figure 5, red</p>
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		<p>squares) with elevated concentrations at the beginning of the short-term calibration (Attachment 4, Figure 5, green triangles), the concentration would once again approach the previously predicted shape overpredicting the rate of recovery for the period after 2010. A slight shift in the composition, an increase in the detrital POC partition coefficient, and/or additional resuspension in erosional areas may produce large changes in the predicted contaminant concentration values. Additional analyses should be presented to support the argument that the existing CFT model framework cannot be used to reproduce the data, and if necessary the framework should be modified to handle partitioning to non-cohesive particles.</p>
563	<p>Section 4.2, page 38, third paragraph and Section 4.2.1, page 39</p>	<p>Carroll et al. (1994) reports the f_{OC} on particles of different sizes in the Hudson River. The f_{OC} values of 6% and more on particles larger than 293 μm (Carroll et al. 1994, Table 1) make it unlikely that these were sand particles with OC coatings. Di Toro et al. (1991) summarizes data from Prah (1982), including measurements of f_{OC} on sand-sized particles, which were segregated based on density. Sand-sized particles with densities greater than 1.9 g/cm³ had an f_{OC} of between 0.2% and 0.4%, while the f_{OC} of lower-density sand-sized particles exceeded 10%. It would be inconsistent to model the transport of low-density, high-f_{OC} particles using the non-cohesive transport equations in the ST model because these lower-density particles would not be transported as bedload in the same way as the sands that are represented in the ST model non-cohesive solids classes.</p> <p>The vast majority of non-cohesive solids transport in the LPR is present as bedload rather than suspended load, and adding bedload to the CFT model would require additional model development and necessitate smaller time steps due to the faster settling velocity of the non-cohesive particles. If this process is important, as suggested in the text, consideration should be given as to how partitioning to sands could be incorporated into the model and represented appropriately without resulting in unacceptable simulation times. Refer to Comment No. 562 for additional concerns with the CPG's analysis to support partitioning to cohesive solids. Please propose a</p>

		solution to address these concerns with the representation of partitioning within the CFT model and revise the text to reflect any actions taken to revise the model.
564	Section 4.2.2, pages 39 through 40	The analysis presented in this section attempts to estimate the contaminant concentrations that would have been calculated if partitioning to non-cohesive solids were included in the model. The results presented are computed by adding contaminant mass to the sediment bed. The contaminant concentration on non-cohesive solids is assumed to be proportional to the concentration on cohesive solids. This approach is not reasonable because the CFT model results are based on initial conditions and external inputs that already account for the total contaminant mass, and the increment added to represent contaminants sorbed to non-cohesive solids represents an artificial source of new contaminant mass. Please revise this analysis to eliminate the invalid creation of contaminant mass.
565	Attachment 1, Figures 3 through 22	Please adjust the figure scales so that lower contaminant concentrations can be distinguished from zeroed areas. Please include additional breaks that distinguish zero separately, and add greater resolution at the low end of the scale. The scale should include breaks for relevant human health and ecological risk screening values and the estimated background concentration.
<u>No.</u>	<u>General Comments – Appendix P</u>	
566	It was inferred that the “Bioaccumulation Model Calibration Report,” delivered to EPA on July 2, 2015, is Appendix P of the Draft RI Report. This document should be labeled correctly as Appendix P – Bioaccumulation Model, and the appendices delivered with this document should be labeled as attachments consistent with the approach used for the other Draft RI appendices.	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix P</u>
567	Section 2.3, page 4, second paragraph, third sentence	It is not necessarily true that “the mechanistic model represents a more realistic picture of bioaccumulation.” Even if all of the mechanistic processes are contained within a model, if the rates governing those processes are not properly set, a mechanistic model can provide less

		accurate predictions than a simpler empirical model would. Please remove this statement from the document.
568	Section 3.1.4, page 11, Figure 3-3	The fish feeding guild classifications presented in Figure 3-3 are not consistent with the dietary preferences of bass in the draft bioaccumulation model presented to EPA. In the model, bass are set to consume an equal amount of planktivores as small forage fish (each category represents 40% of their overall diet), whereas the data shown in Figure 3-3 indicate that there are 11 times as many small forage fish and “invertivores/omnivores” as planktivores (88% vs. 8%, respectively) in the LPRSA. Please correct the bass dietary preferences in the model.
569	Section 3.1.5, page 12, second paragraph	The particulate ventilation construct is an empirical “black box” used to make the carp model fit the data. A chemical-specific particulate ventilation parameter is especially inappropriate as there is no basis for assuming different ventilation rates for different chemicals. Carp bioturbation is, however, known to stir up deeper and more contaminated sediment, up to 16.7 cm deep. Huser et al. (2015) determined that “the sediment mixing depth was at least 2.5 times greater in areas with carp (13.0 ± 3.7 cm) than in areas from which carp had been excluded (5.0 ± 1.2 cm) using exclosures.” Please recalibrate the model without the particulate ventilation construct, but including carp feeding on deeper sediments, and revise the text accordingly.
570	Section 3.2.1, page 14, last bullet	The text lists “Near-bottom particulates” as a physical medium for inclusion in the bioaccumulation model. This is later defined (in the last bullet on page 16) as particulate matter in the bottom layer of the water column, rather than the modeled “fluff layer.” It is unclear why the fluff layer, which was added to the CFT model, is not being used in the bioaccumulation model by organisms feeding on that layer. Please revise the text to clarify why, in a mechanistic model where the layer used for feeding by a given organism is explicitly modeled, the water column above that layer is considered a better representation of contaminant concentrations.

571	Section 3.2.2, page 16, first bullet, first sentence	<p>Using the top 2 cm of the sediment as the physical exposure medium is not acceptable. The available data to constrain predicted concentrations within this layer (water column concentrations and 15-cm concentrations) are not direct measurements of 2-cm concentrations.</p> <p>Furthermore, it appears likely that multiple alternative calibrations within these constraints are possible (e.g., higher or lower 2-cm sediment concentrations could be calculated and the model could still be predicting reasonable water column and 15-cm concentrations). Please recalibrate the model using a different physical exposure medium for sediment and rewrite this section accordingly.</p>
572	Section 3.2.2, page 16, first bullet, last sentence	<p>The depth of mixing, the rate of mixing, and the depth of exposure are not directly constrained by the site-specific data collected for the RI (refer to Comment Nos. 535, 538, 559, 560, 569, and 611). The CPG should present CFT sensitivity analysis results for a range of mixing depths and rates, and the depth of exposure should be revised to reflect the depth determined as a result of the currently ongoing dispute resolution. The final mixing depth, mixing rate, and vertical variation in particle mixing rate should reflect the results of the dispute resolution as well as the sensitivity analysis. This sentence should be revised to reflect the resulting changes in the CFT calibration and exposure depth.</p>
573	Section 3.2.3, page 17, item 4 and second paragraph, first sentence	<p>The analysis of relative abundance of the three benthic invertebrate groups, detailed in Appendix E, is based on combining non-site-specific species weights from Chesapeake Bay with local counts. This is a flawed analysis as discussed in the comments for Appendix E.</p> <p>Furthermore, the conclusion that “DETs comprise the majority of the benthic biomass in the LPRSA” is based on a highly variable and uncertain set of biomass data, especially for <i>Corbicula sp.</i> (Asian mud clam). For RM 4 to RM 13, the data suggest that detritivores and deposit feeders are roughly equal in biomass, as presented in the analysis in Comment No. 614.</p>

574	Section 3.2.5, page 21, first paragraph, first sentence	<p>The plots presented in Attachment 4, Figures 6a through 6e, developed by HDR show 2,3,7,8-TCDD fish tissue data by RM for forage fish, carp, white perch, American eel, and bass. As illustrated by these plots, for many species there are strong relationships between 2,3,7,8-TCDD concentration and RM, which makes the use of a single “modeling area” for each species inappropriate. Within the attached figures, central tendencies used in the CPG’s modeling are plotted as horizontal lines.</p> <p>As stated in the RI Report Executive Summary (page ES-4, third paragraph, first sentence), sediment concentrations are also heterogeneous and have a relationship with RM (“High surface sediment 2,3,7,8-TCDD concentrations are rare upstream of RM 12...”). Choosing not to calibrate the bioaccumulation model with RM bins results in the elimination of valuable information and unnecessarily simplifies the modeling, producing a model that is unacceptable for risk evaluation. Please revisit the model calibration using RM bins or another appropriate spatial segregation and reproduce this section of the report accordingly.</p>
575	Section 3.3.1, page 31, Table 3-3	<p>The statement that “CYP450 1A expression (CYP450 1A1 is the most important enzyme in TCDD metabolism for vertebrates) is not known to occur in benthic invertebrates” suggests that using fish-derived 2,3,7,8-TCDD metabolism rates for invertebrates results in higher metabolism estimates than would be expected based on known enzyme metabolism processes. However, even when calibrating the model using these high metabolism values, the CPG model for deposit feeders is significantly overpredicting tissue concentrations, as evidenced by Figure 10-16 of the cited HydroQual (2007) document, reproduced below. The CPG model produces biota-sediment accumulation factors (BSAFs) for worms (and worm contaminant concentrations) that are much too high. The CPG’s Log BSAFs for worms (0.34 to 0.41 Log kg-OC/kg-Lip) are plotted as a red line above the Log BSAFs observed and modeled in the New York-New Jersey Harbor Estuary in the figure below. The BSAFs predicted by the model are an order of magnitude higher than those observed. Other lines of evidence regarding the overprediction of contaminant concentrations in benthic</p>

		<p>invertebrates are presented in Comment No. 595. Please revise the bioaccumulation model such that the BSAFs for benthic invertebrates are in line with observed data.</p> <hr/> <p>Diox Deriv and R Figs</p>
576	Section 3.3.1, page 31, Table 3-3	<p>The rationale for benthic invertebrate metabolism of 2,3,7,8-TCDD states that the nature of metabolism of 2,3,7,8 TCDD is unknown and that there could be inefficient transfer of dioxin/furan congeners from sediment or that worms may be able to metabolize dioxin. In other words, the processes are highly uncertain, which calls into question the choice of a mechanistic model for these benthic compartments. Furthermore, no comparisons were made between tissue data and model results within the model calibration. The K_M parameter was used as an open parameter due to process-level uncertainty, but whether this parameter was properly selected is not possible to assess without comparing the model results to benthic data. As discussed in Comment Nos. 575 and 595, it appears that the benthic model is dramatically overpredicting contaminant uptake for deposit feeders. Please consider revising the model calibration to utilize a BSAF for this model category using either data available from HydroQual (2007), cited within the report, or site-specific data from the worm bioaccumulation tissue tests, especially if the mechanistic model cannot be revised to produce reasonable BSAF predictions.</p>
577	Section 3.3.1, page 33, Table 3-4	<p>The range of RMs contained within each calibration domain is inappropriate given the gradient in contamination acknowledged within the RI Report. As stated in the RI Report Executive Summary (page ES-4, third paragraph, first sentence), "High surface sediment 2,3,7,8-TCDD concentrations are rare upstream of RM 12..." In all three of the RM bins presented in Table 3-4,</p>

		<p>concentrations upstream of RM 12 are averaged with data from between 5 and 12 RMs downstream of RM 12.</p> <p>Furthermore, the CPG seems to have conflated the concept of “species range” and “home range” within this analysis, modeling each entire “species range” as a single “box.” To define the appropriate range for each species would involve a combination of species-specific “home range” information and information about differing sediment concentrations. Furthermore, even if a species has a relatively wide home range, animal tissue data suggest that these species are sensitive to the zone in which they are caught (refer to the data presented in Attachment 4, Figures 6a through 6e). Therefore, looking at the trends in tissue data can help to define the bins in which the animal tissue data should be compared to model predictions.</p> <p>The CPG’s research regarding the “home range” for each species and how this translates into the resulting assumptions regarding the exposure range that should be applied to each organism should be clearly presented in this report. Given the strong trends by 2-RM bins for many species (especially for 2,3,7,8 TCDD), the default approach should be to present model-to-data comparisons on this basis, unless the CPG is able to make a case that wider bins are appropriate due to a lack of significant differences in the data.</p> <p>Please recalibrate the model using multiple sediment bins with greater spatial refinement for each calibrated organism to provide a more accurate and tightly constrained model, and revise the text accordingly once this recalibration is performed.</p>
578	Section 3.3.1, page 34, second paragraph, first sentence	<p>The addition of an empirical “species-specific particulate ventilation constant” to the bioaccumulation model for carp indicates that the carp model is not working without this factor and provides another open parameter for what is already seen as an unconstrained model. This parameter seems to be unique to this model, as EPA’s team has not encountered a “particulate ventilation factor” in any other bioaccumulation modeling exercise. Furthermore, the use of a different factor for different chemicals loosens any constraint on the carp calibration. Given that only a single model-to-data comparison is used for each organism, carp</p>

		<p>can be successfully calibrated to any exposure scenario simply by changing the input for each chemical being modeled.</p> <p>The fact that carp are not accumulating enough contaminant in the model without this empirical factor likely means that carp are being exposed to more contaminated, deeper sediment than what is being modeled by the CPG. Alternatively, the process of carp feeding may be stirring up more contaminated, deeper sediment that is not appropriately accounted for in the shallow water predictions. Huser et al. (2015) indicates that carp can stir up sediment as deep as 9.3 to 16.3 cm below the surface.</p> <p>The use of the empirical “particulate ventilation constant” acknowledges that carp will stir up sediment and have a concurrent effect on sediment remobilization. In the model, however, this affects the bioaccumulation prediction for carp alone, with no effect on water column concentrations or concentrations predicted in any other organism. In addition, this empirical factor, for which the CPG acknowledges “no information is available regarding the correct parameter value,” allows the CPG to simply increase the amount of contaminant that carp are receiving from the water column, whether feeding preferences are properly set or not (refer to Comment No. 569).</p> <p>Finally, most bioaccumulation models assume that chemicals sorbed to POC and even chemicals sorbed to DOC are not bioavailable through the gills. For example, Arnot and Gobas (2004) state that “If associated with particulate or dissolved organic matter, the chemical is believed to be unavailable for uptake via diffusion into organisms.” Therefore, stretching the model to assume that chemicals sorbed to POC are bioavailable through the gills represents a significant departure from accepted bioaccumulation modeling practice.</p> <p>Please either recalibrate the model without this particulate ventilation construct or provide sufficient literature or experimental data to support this novel modeling approach.</p>
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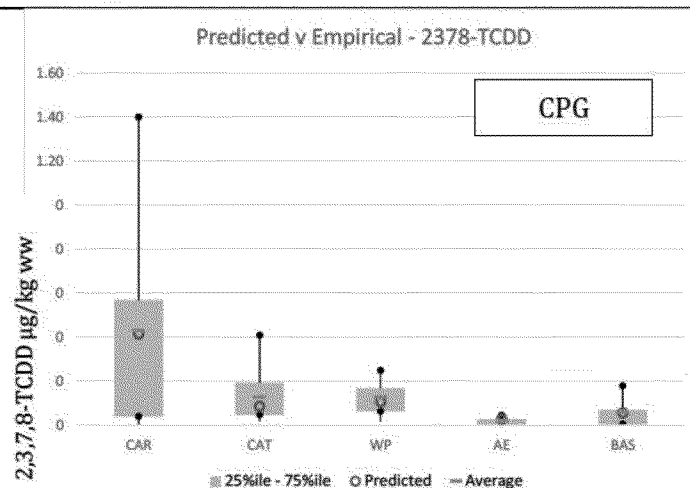
579	Section 3.3.2, pages 36 and 37, Table 3-8	Table 3-8 and the derived dissolved oxygen (DO) saturation averages shown at the bottom of this table raise the question as to why a single study area-wide or habitat-wide calibration statistic was used for all modeled organisms when there are RM-specific differences in DO saturation (as well as f_{oc} , contaminant concentrations, and fish tissue concentrations). For example, the study area-wide average DO saturation of 80% shown in Table 3-8 is unlikely to accurately reflect conditions in a river in which DO saturation is 97% at RM 14 and 66% at RM 1. Please recalibrate the model using greater spatial refinement and revise the text accordingly once this recalibration has been performed.
580	Section 3.3.3, page 39, Table 3-9	It is likely that the dietary absorption efficiency (AE) of non-lipid organic carbon (NLOC) needs to be set lower for benthic invertebrates given the significant overprediction of the model for this category. As discussed in Comment Nos. 575, 576, and 595 , the BSAFs are too high. The unconstrained metabolism rate chosen for 2,3,7,8-TCDD in invertebrates must also be considered when setting this parameter, as these two parameters affect the same outcome; therefore, the AE cannot be calibrated without also reconsidering the calibration of the metabolism rate. Please either recalibrate or replace the benthic organism model with these considerations in mind.
581	Section 3.3.3, page 39, Table 3-9	Using literature-based weights rather than site-specific weights for benthic invertebrates is not defensible as the CPG previously made the case, during the February 6, 2015 meeting, that benthic organisms are unique in the LPR and that phenotypic differences make them much smaller than in other locations. At a minimum, the CPG must acknowledge the uncertainty in this parameter and add an acceptable range within Table 3.9. Furthermore, the CPG should test assumed organism weights as part of the model uncertainty analysis, including the assumed benthic organism composition for predators (currently assumed to be dominated by detritivores).
582	Section 3.3.3, page 41, Table 3-9	Carp lipid fraction and fraction of porewater ventilated were set to their nominal values in the model calibration. Rather than adding an empirical “particulate ventilation constant” to the model because this compartment would

		not calibrate using these nominal values, the CPG should first attempt to calibrate this species using the uncertainty within the parameters for the existing peer-reviewed Arnot and Gobas (2004) model. It is likely that increasing the depth of sediment exposure for carp would improve the calibration. Please recalibrate the model using different values for the carp parameters and without the use of the “particulate ventilation constant,” and revise the table and text as necessary based on this recalibration.
583	Section 3.3.3, page 44, Table 3-11	The model calibration has assigned detritivores to 0% feeding on the sediment bed. Polychaetes such as <i>Nereis virens</i> are included in this group, and feeding preferences of polychaetes suggest some sediment feeding. Nielsen et al. (1995) determined that <i>Nereis virens</i> can be considered benthic feeders, stating that “They use their powerful jaws as predators or scavengers...or obtain nourishment by swallowing the uppermost sediment layer with its content of detritus and microbenthic algae.” Therefore, please revise the model calibration and Table 3-11 to assign detritivores a non-zero sediment bed feeding preference.
584	Section 3.3.3, page 47, Table 3-11	Catfish are carnivorous bottom feeders that eat benthic invertebrates such as worms that reside in the sediment. Assigning the deposit feeder proportion of prey to 0% with no possible calibration of non-zero consumption in this category is not supported by the data. For example, Tófoli et al. (2013) indicates that catfish are known to feed on deposit-feeding oligochaete worms. Please recalibrate the model using a non-zero proportion of deposit feeders consumed by catfish and revise the table once this recalibration has been performed.
585	Section 3.3.3, page 50, Table 3-11	Refer to Comment No. 568 . The dietary preferences of bass are not consistent with the abundance data presented in Figure 3-3. Based on Table 3-11, bass are set to consume an equal amount of planktivores (filter-feeding fish) as small forage fish (each representing 40% of their overall diet), whereas the data presented in Figure 3-3 indicate that there are 11 times as many small forage fish as planktivores (88% vs. 8%, respectively). As stated in the dietary preference rationale text in Table 3-11, “the actual dietary portions are likely based on the availability and abundance of these types of small fish in the LPRSA.”

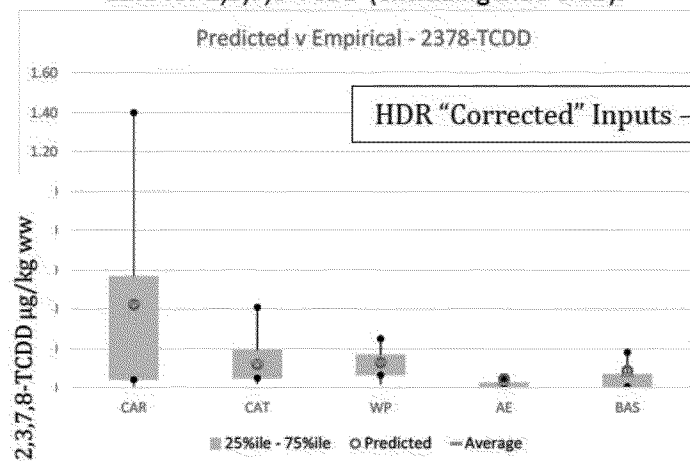
		Please revise the bass feeding preferences to properly reflect prey availability.
586	Section 3.3.4, page 51, first sentence	Data passed to the bioaccumulation model were averaged over the calibration period of 2011-2013, as stated in Appendix C: Use of CFT Model Data For Calibration. Because empirical tissue samples were collected from the LPRSA in 2009/2010, the calibration period should not include 2011 sediment exposure concentrations, which include post-Hurricane Irene data. Please recalibrate the model using a 2009/2010 CFT calibration or remove post-Hurricane Irene exposures from the data passed to the bioaccumulation model.
587	Section 3.3.4, page 52, second paragraph and Table 3-12	Please explain why whole-body samples for bullhead (n=6), shad (n=3), small forage fish (n=4), mummichog (n=18), perch (n=22), and benthic invertebrates (n=19) were omitted from the weight-of-evidence calibration approach. These species must be included within the calibration approach.
588	Section 3.4.1.2, page 59	<p>The CPG's calibration technique is not typical and does not prove that a linked-model setup would be properly calibrated. The calibration applied uses a steady-state model that averages 3 years of contaminant exposure results. Without further testing, the CPG wishes to apply this model to a time-series model to evaluate rates of change within biota.</p> <p>A calibration to a steady-state model could be a reasonable first step in model calibration. However, a required next step would be to display time-series results from the linked CFT and bioaccumulation models and to compare these results to all empirical data <u>at the time these data were collected</u>. Time-series results would also be required to assess whether fish tissue predictions are reasonable in terms of rates of change and variability in tissue concentrations.</p> <p>Furthermore, as stated in Comment No. 586, EPA has significant concerns about the spatial and temporal averaging of input data and fish tissue data. For example, the CFT model data is first averaged monthly and then averaged again over the 3-year period of 2011 to 2013. This results in a comparison of model results that include</p>

		<p>post-Hurricane Irene sediment concentrations in the modeled contaminant exposures. However, all tissue data were collected pre-Hurricane Irene.</p> <p>Please present time-series results as part of the model calibration report. If a steady-state component is also to be included, comparisons of sediment exposure to tissue concentration must be as temporally synoptic as possible, as stated in Comment No. 586.</p>
589	Section 3.4.1.2, page 60, last paragraph, last sentence	EPA requires that the calibrated non-steady-state model be tested prior to determining the calibration as final, among many other requirements (such as consideration of additional empirical data and the use of more complex spatial binning). Therefore, please remove the statement that “the calibration was considered final.”
590	Section 3.4.2.1, page 62, Table 3-14	Calibration of invertebrate parameters such as AE and K_M without any comparison to invertebrate data (BSAFs, site-specific data, or otherwise) means that these parameters were calibrated solely to achieve model predictions that match fish tissue concentration data. Given uncertainties in feeding preferences, this portion of the model is overly uncertain. As discussed in Comment Nos. 575 and 595 , EPA does not consider that the benthic invertebrate model is reasonably calibrated and directs that this portion of the model be reworked or replaced (including comparisons to BSAFs or observed data) to produce reasonable BSAF predictions.
591	Section 3.4.2.1, page 63, Table 3-14	An empirical factor that is chemical-specific and site-specific, the “particulate ventilation constant,” cannot be appropriately added to the carp model. This parameter has no numerical basis in data or literature studies and allows the CPG to simply set the carp tissue concentrations to whatever the data show, regardless of whether the exposure pathway for this species is appropriately represented, as discussed in Comment No. 578 . Please recalibrate the model without this construct.
592	Section 3.4.2.2, page 66	The calibrated model performance is reasonable based on the metrics chosen; however, the breadth of metrics chosen is not wide enough, certain tissue comparisons have been needlessly omitted (invertebrates and forage fish), and other tissue comparisons have been rendered

		<p>meaningless by the selection of excessively wide ranges for spatial and temporal averaging. Furthermore, an investigation by HDR uncovered many deficiencies with the model linkage procedure, including the equations used for averaging and calculation of bioavailable concentrations in the water column. Because the model does not account for chemicals sorbed to DOC in the water column, it dramatically overestimates the bioavailable water column concentration. In addition, EPA does not accept a 2-cm averaging depth or the inclusion of post-Hurricane Irene water column concentrations in the model calibration for comparison to pre-Hurricane Irene tissue data.</p> <p>As proof that this model calibration is not unique or well constrained, HDR produced “corrected” bioaccumulation model inputs based on the CPG’s CFT model results from July through September 2009 (a better temporal match to much of the fish-tissue data) and ran these inputs through the bioaccumulation model. “Corrections” included setting the sediment averaging depth to 15 cm and correcting bioavailable water concentrations (refer to Comment Nos. 361 and 541; HDR used an a_{DOC} of 0.08 and K_{OW} of 6.35 to compute freely dissolved contaminant rather than assuming no partitioning to DOC). With no additional calibration, the CPG model produced nearly identical results. The original underprediction in the sediment bed, using an averaging depth of only 2 cm, was nearly completely offset by the original overprediction in the water column concentrations due to incorrect partitioning and the inclusion of post-Hurricane Irene data. Diagrams of the “corrected” model results compared to the original results are shown below.</p>
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CPG calibration with CPG inputs; same results shown in Figure 3-18. Empirical and model-predicted whole-body data for 2,3,7,8-TCDD (excluding blue crab).



CPG model with HDR "corrected" inputs (CFT results for July through September 2009)

Another test was run using the same 3-year period and spatial averaging as the CPG, but again "correcting" the sediment depth to 15 cm and the calculation for the water column bioavailable concentration. Remarkably, the calibration again looks nearly identical, as shown in the figure below. Water column concentrations in this new test were higher than the 2009 time period due to the inclusion of storm events. However, averaging water temperature over the entire time period resulted in a lower chemical uptake that offset the higher exposures. In summary, the CPG must present a model calibration in which water temperatures are dynamic and in which tissue calculations are compared with model predictions for an appropriate time period. It is clearly possible to

		<p>calibrate this model with deeper sediment exposure depths.</p> <p>Predicted v Empirical - 2378-TCDD</p> <p>HDR "Corrected" Inputs – 3-</p> <p>2,3,7,8-TCDD µg/kg ww</p> <p>25thile - 75thile O Predicted — Average</p> <p>G model with HDR "corrected" inputs (3-year period)</p> <p>Based on all of these considerations, the evidence presented by the CPG is not sufficient proof that this model calibration is unique or appropriate. The model requires additional calibration and testing at different spatial and temporal resolution prior to its application for remedial alternatives.</p>
593	Section 4.1.1.1, page 73, Table 4-1	<p>The model significantly overpredicts tissue concentrations in small filter-feeding fish. This is likely associated with model overpredictions of water column concentrations due to the inclusion of Hurricane Irene in the calibration period, and could also be related to other parameters and feeding preferences chosen. Please recalibrate the model to correct this overprediction.</p>
594	Section 4.1.1.2, page 73, last sentence (continued on page 74)	<p>The statement that "the small home range and foraging area of these small forage fish mean that the data are not necessarily representative of concentrations that would be present in small forage fish throughout the LPRSA" supports EPA's assertion that spatially averaging all tissue concentrations through the entire LPRSA is a flawed calibration procedure and results in the omission of data and RM trends that would be instructive for model calibration. Please recalibrate the model to include small forage fish and average data in appropriate spatial areas (perhaps within 2-RM bins). Refer to Comment No. 577 for more specific direction regarding home range determination and the model-to-data comparison.</p>

<p>595</p>	<p>Section 4.1.1.4, page 75, second paragraph, third sentence</p>	<p>As noted in this section, the benthic invertebrate model is dramatically overpredicting 2,3,7,8-TCDD concentrations based on comparisons to laboratory data. This is just one of several lines of evidence that the benthic invertebrate model for 2,3,7,8-TCDD is not properly working and should potentially be replaced by a BSAF approach. Other lines of evidence that the benthic invertebrate model is significantly overpredicting 2,3,7,8-TCDD concentrations include:</p> <ul style="list-style-type: none"> • Comparison to BSAF data from HydroQual (2007) and the Canadian Council of Ministers of the Environment (2001). As noted in Comment No. 575, model predictions result in a BSAF of 2.0 to 2.5 for 2,3,7,8-TCDD. The Canadian Council of Ministers of the Environment (2001) indicates that for 2,3,7,8-TCDD, observed BSAFs range from 0.03 to 0.85 in fresh water. Multiple literature sources were examined (e.g., Loonen et al. 1997; Muir et al. 1992; Servos 1996), and none support BSAFs greater than 2.0 for benthic invertebrates; rather, most suggest that this number should be lower than 1.0. • Predictions of 2,3,7,8-TCDD concentrations in deposit feeders are higher than all fish tissue predictions, including carp. • As shown in Table 4-4, model predictions of 2,3,7,8-TCDD in benthic invertebrates are too high by a factor of 23 to 33 when compared to mean concentrations from the bioaccumulation investigation, and by a factor of 4 to 10 when compared to the maximum concentrations measured. The statement that these overpredictions “could be an indication that laboratory worms had a more effective mechanism for limiting exposure or uptake or eliminating 2,3,7,8-TCDD than that assumed in the model” is an admission that available data suggest that the model assumptions are not accurate. <p>Therefore, the extremely high uptake predicted for deposit feeders in the model is refuted by the best available data. Please recalibrate the model using an</p>
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		approach that will result in BSAFs within guidelines found in the literature and revise the text and table accordingly.
596	Section 4.1.2.2, page 78	Small forage fish should be included in the primary model calibration and not relegated to the uncertainty analysis. The 2,3,7,8-TCDD data in Table 4-6 indicate that fish tissue data vary by RM bin, so model-to-data comparisons should be evaluated on that basis. Please recalibrate the model to include small forage fish and revise the text and table accordingly.
597	Section 4.1.2.2, page 79, Table 4-6	As discussed in Comment No. 596 , Table 4-6 shows that there is a relationship between small forage fish tissue data and RM, as well as a relationship between observed sediment concentrations and RM. Strong relationships with RM have also been observed in tissue data for bass, carp, and American eel, as shown in Attachment 4, Figures 6a through 6e (refer to Comment No. 574). For all of these organisms, a calibrated model must be produced that is a function of RM; a single model calibration for the entire LPRSA is not acceptable. Please recalibrate the model taking these relationships into account and revise the text and table accordingly.
598	Section 4.1.2.3, page 80, first paragraph, fifth sentence	It is understandable that increasing the depth of exposure, without changing anything else within the calibration, results in higher fish tissue concentrations and negatively affects the existing calibration. However, as discussed in Comment No. 592 , correcting the averaging period and the water column bioavailable fraction would result in little additional required calibration and reasonable tissue concentrations, while assuming a deeper sediment averaging depth. This type of “one parameter at a time” sensitivity analysis does not provide any additional information in the calibration report; Section 4.1.2.3 should be removed.
599	Section 4.1.2.4, page 82, second sentence	Removing the near-bottom particulate layer from the calibrated model and noting that the results differ does not provide useful information. There is no evidence that the model cannot be calibrated without the addition of the near-bottom particulate layer and no observed data for this layer, making it another unconstrained portion of the model with regards to chemical concentrations sorbed

		to this layer. Furthermore, it is unclear why the near-bottom particulate layer is used as a surrogate for the “fluff layer,” which is explicitly mechanistically modeled within the CFT model (refer to Comment No. 570). This type of “one parameter at a time” sensitivity analysis does not provide any additional information in the calibration report; Section 4.1.2.4 should be removed, or an alternative calibration without the fluff layer should be presented.
600	Section 4.1.2.5, page 84, Figure 4-2	Figure 4-2 is not an adequate replacement for comparisons of fish tissue predictions to fish tissue observations within RM bins. Please produce such figures so that the model calibration can be evaluated for each stretch of the river. As mentioned in Comment No. 574 , there are strong trends in fish tissue and sediment concentrations by RM. The favorable comparisons shown in Figure 4-2 could be a result of the model significantly overpredicting observed data in some reaches and significantly underpredicting in other reaches, resulting in what looks like, but in reality is not, an acceptable model calibration when everything is averaged together.
601	Section 4.1.4, page 88, Table 4-11	As is the case with all tissue-to-data comparisons used for the model calibration, Table 4-11 ignores the strong correlation between RM and fish tissue concentrations (refer to Comment No. 574) and instead presents ratios between the entire habitat-area exposure concentrations and the average fish tissue concentrations. Please redo this analysis by comparing fish tissue concentrations with spatially relevant sediment concentrations, which also have strong trends by RM, and revise the table accordingly after this analysis is complete.
602	Section 4.1.4, page 88, second paragraph, and page 89, first sentence	The “high ratio of tissue-to-sediment concentrations for carp” could be a function of the carp feeding on sediments, including those deeper than 2 cm, rather than near-bottom particulates. This would indicate a closer relationship between carp tissue and sediment concentrations than is predicted by the model. Furthermore, the statement that “if the selective feeding hypothesis were true, the model would tend to underestimate the effectiveness of a targeted remedy designed to remove higher concentrations of 2,3,7,8-TCDD

		in surface sediment” acknowledges that the empirical “particulate ventilation constant” for carp could have an effect on the selected remedy and has no mechanistic basis (refer to Comment Nos. 569 and 578). Please replace this empirical factor with other calibration methods.
603	Section 4.1.5.1, page 90, first bullet, second sentence	The statement that “The average tissue concentration from these six samples can be considered as representative of the bass modeling area, because the samples were collected using an unbiased sampling design” ignores the strong trend by RM in bass samples as shown in Attachment 4, Figure 6e . Therefore, the average concentration cannot be considered representative of the entire modeling area; rather, the modeling area must be spatially separated. Refer to Comment No. 577 for specific recommendations.
604	Section 4.1.5.2, page 91, third sentence	The dietary preferences of smaller eels are quite different from those of larger eels, as documented in Appendix H. The fact that “the chemistry data for the smaller eel were not substantially different from those for the larger eel” may be due to the smaller eels, which feed directly from the sediment, consuming more contaminated sediments for a shorter time (based on Table 2 in Appendix H). Because their dietary compositions are notably different, small eels would be expected to respond differently than larger eels to remedial alternatives that clean up the sediment faster than the water column, or vice versa. Therefore, please recalibrate the model to explicitly include small eels and revise the text accordingly.
605	Section 4.2.2, page 96	<p>The water temperature sensitivity analysis is incomplete as it does not include temperatures over 17.5 degrees Celsius (°C). The CPG model has a threshold of 17.5°C for increased growth dilution effects, and water temperatures in the LPRSA can exceed 17.5°C during summer months. Please reproduce the sensitivity analysis using temperatures above this threshold. Furthermore, consider replacing the hard temperature threshold for growth dilution effects with a curve, as this formulation is not realistic.</p> <p>Water temperature is a sensitive value in these models as it affects bioenergetics. Averaging water temperature over</p>

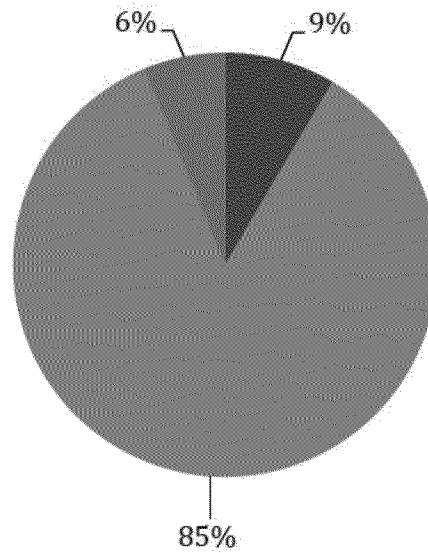
		3 years is not an appropriate way to calibrate a bioaccumulation model. Time-series model results must be evaluated as part of this model calibration; refer to Comment No. 588 .
606	Section 4.2.3, page 99, first sentence	The statement that “tissue concentration predictions are sensitive to K_M , particularly for species for which the K_M distribution range is large” indicates that the model is not well constrained. Based on this highly uncertain parameter, the CPG model can easily be made to fit observed fish tissue concentrations given a large variety of exposure concentrations and pathways, whether they are properly represented or not. Please explore alternative calibrations for this parameter and consider how these would affect different remedial alternatives.
607	Section 4.2.5, page 103, first paragraph, fourth and fifth sentences	All of the sensitivity analyses presented in this report are “marginal sensitivity analyses” that have limited value in terms of evaluating the model calibration. However, these sensitivity analyses generally suggest that many alternative model calibrations are likely possible. For example, if the food web was improperly specified or if feeding exposure depths were improperly set, there are enough uncertain and sensitive variables that the model could be made to produce a “reasonable” calibration despite its underlying flaws. Please replace the marginal sensitivity analyses in this report with a discussion of alternative calibrations.
608	Section 4.2.5, page 106, first bullet	The fact that changes to the input chemical concentration in sediment “had a relatively small impact on the overall model results” is due to feeding preferences that were based on a flawed analysis of the biomass of deposit feeders as compared to detritivores, as discussed in comments on Appendix E and Appendix H (refer to Comment Nos. 614 and 615). Therefore, this sensitivity analysis, suggesting that the model is not sensitive to changes in chemical concentrations in sediment, is itself flawed. Please revise this analysis and the associated text once the necessary changes regarding the feeding preferences are made.

609	Section 5, page 108, second paragraph	<p>As discussed in previous comments, there are significant flaws in the bioaccumulation model calibration approach, the data averaging techniques, the parameters selected, and the addition of unproven empirical factors to the model. Please recalibrate the model using, at a minimum,</p> <ol style="list-style-type: none"> 1. Time-series model results (Comment No. 588) 2. RM bins for fish tissue exposures and comparisons to observed data (Comment No. 577) 3. Deeper sediment exposure depths (Comment No. 592) 4. Appropriate selection of time periods when comparing tissue concentrations to empirical data (e.g., post-Irene exposure predictions should not be compared to pre-Irene tissue samples; Comment No. 586) <p>After these changes are made, EPA will reevaluate whether the bioaccumulation model “is a reliable tool for the evaluation of remedial scenarios.” Until this determination is made, please remove this text from the report.</p>
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix P, Appendix B</u>
610	Appendix B, Section 2.3.4, page 10, Equations 20 and 21	<p>More information about the basis for the model’s growth-rate allometrics is required. The Arnot and Gobas (2004) paper and the citations it references in support of its allometric estimations do not clarify whether these estimations are based on fish, invertebrate, or mixed data, nor the range of weights to which these estimations should apply. Allometric equations can be in error when they are applied at the edge of their domain or when extrapolating below measured organism weights. For benthic invertebrates, given the poor performance of the model discussed in Comment No. 595, the absence of site-specific organism weights, and uncertainty as to whether this allometric formulation was derived using invertebrate data, species-specific growth rates should be used in place of the Arnot and Gobas (2004) estimations.</p>
<u>No.</u>	<u>General Comments – Appendix P, Appendix C</u>	
611	Please modify the approach to passing data from the CFT model to the bioaccumulation model by making the following changes (refer to Comment No. 361 and Attachment 3):	

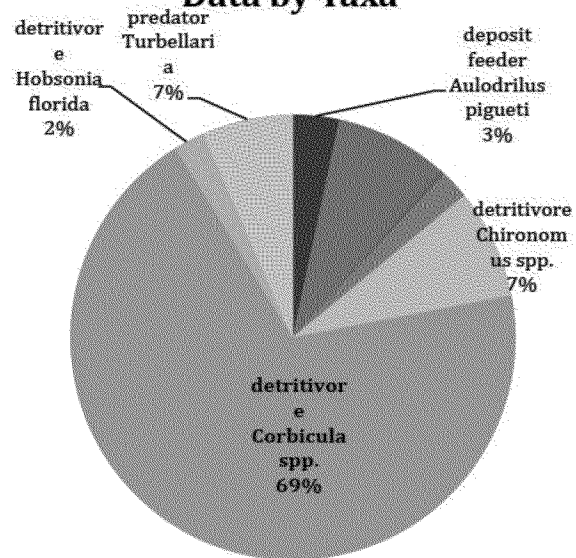
	<ul style="list-style-type: none"> • Calculate particulate concentrations for each layer of the water column by dividing volumetric sorbed chemical concentrations by volumetric solids concentrations prior to averaging over depth. The current approach of averaging the volumetric sorbed chemical concentrations and volumetric solids concentration over depth first, then dividing the two averages, does not reproduce the depth-averaged particulate concentration. • Use CFT model outputs for the “fluff layer” instead of the bottom layer of the water column for modeled species that feed upon the “fluff layer.” • Include partitioning to algae in both the CFT and bioaccumulation models and ensure that representation in the two models is consistent. The current approach assumes no partitioning to algae in the CFT model, but includes partitioning to algae the bioaccumulation model (refer to Comment Nos. 371, 373, 398, 557, and 591). • Include partitioning to DOC in both the CFT and bioaccumulation models and in both the water column and the sediment. Currently, partitioning to DOC is only considered in the sediment in the CFT model (refer to Comment Nos. 536 and 565). • Spatially and temporally average inputs to reflect the patterns in space and time observed in the exposure and tissue data (refer to Comment Nos. 574, 577, 587, 588, 589, 596, 598, 603, and 608). • Average sediment concentrations over an appropriate depth (to be determined as a result of the currently ongoing dispute resolution). 	
<u>No.</u>	<u>Page No.</u>	<u>Specific Comments – Appendix P, Appendix C</u>
612	Appendix C, Section 1, page 1, first bullet	Although monthly averages were provided, those values were averaged into a single 3-year value without weighting based on the number of days in each month, and the time period over which those values were averaged (October 2010 through September 2013) did not match the time of the data collection (August and September 2009 and June, July, and August 2010). The sediment data and the CPG’s model suggest that the exposure concentrations were higher in the period after Hurricane Irene (August 20-28, 2011) than they were when the fish data were collected. Please reevaluate the CFT model predicted concentrations used to calibrate the bioaccumulation model so that the averaging period used matches the time of the data collection and is consistent

		with the duration required to approach steady-state based on the model parameter values chosen.
613	Appendix C, Section 1, page 1, second bullet	The three spatial scales used in the calibration (study area-wide, RM 4 to Dundee Dam, and RM 7 to Dundee Dam) do not reflect the spatial gradients in the tissue and exposure concentration data. Please recalibrate the model using greater spatial resolution, which could be as fine as the eight reaches used in the 2009 Fish and Decapod Tissue Collection and 2010 Small Forage Fish Tissue Collection programs (RM 0 to RM 2, RM 2 to RM 4, RM 4 to RM 6, RM 6 to RM 8, RM 8 to RM 10, RM 10 to RM 12, RM 12 to RM 14, and RM 14 to RM 17.4).
614	Appendix E, Section 4.2, page 13, Figure 1	<p>The food web presented in the bioaccumulation model calibration report is based on the argument that the LPRSA is dominated by detritivores, or “fluff layer” consumers (refer to Comment No. 573). However, the analysis in Appendix E utilizes average weights measured in Chesapeake Bay and combines these data with abundances measured in the LPRSA, as presented in the BERA. Given site-specific differences in organism sizes based on site-specific factors and speciation, this is not an appropriate procedure to estimate biomass.</p> <p>For example, Figure 1 in Appendix E suggests that detritivores make up 85% of biomass in the study area, based on abundances in the BERA. However, this figure does not illustrate that the vast majority of the detritivore biomass is composed of bivalves, specifically <i>Corbicula</i>. The wet weight of these clams can vary dramatically from one location to another based on age and environmental conditions.</p>

Figure 1: Sitewide



Data by Taxa

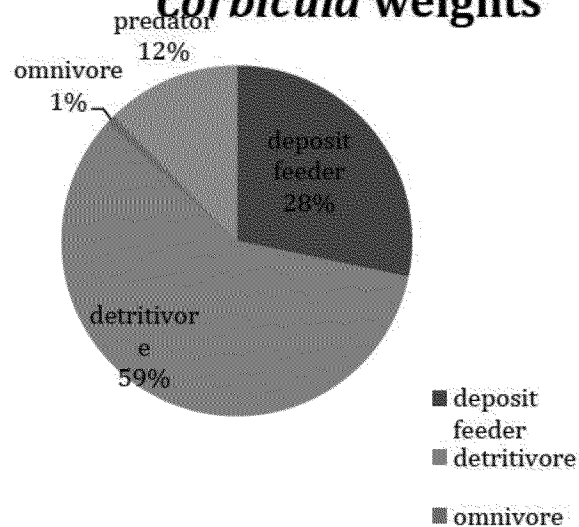


In February of 2015, in a discussion of benthic feeding zones, the CPG presented images and specimens to EPA that suggest that typical clams in the LPRSA are tiny, with a maximum shell width of approximately 5 millimeters (mm), as shown below.

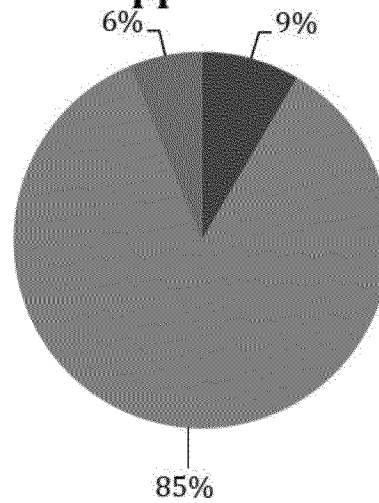


Using this picture and generic weight-to-shell-width data (Helm et al. 2004), the mean biomass of a 4-mm bivalve can be estimated as 17 milligrams (mg), which is over 3 times less biomass than estimated by the CPG using the Chesapeake Bay data. Substituting this alternate wet weight for *Corbicula* into Figure 1 results in the figure shown below.

**Figure 1 using revised
Corbicula weights**



**Figure 1 in
Appendix E**



If the same pie chart were derived using abundance data for RM 4 to RM 13, it suggests that deposit feeders make up 38% of the benthic biomass and detritivores only 40%.

**Figure 1 using revised
Corbicula weights (RM 4 to
RM 13)**

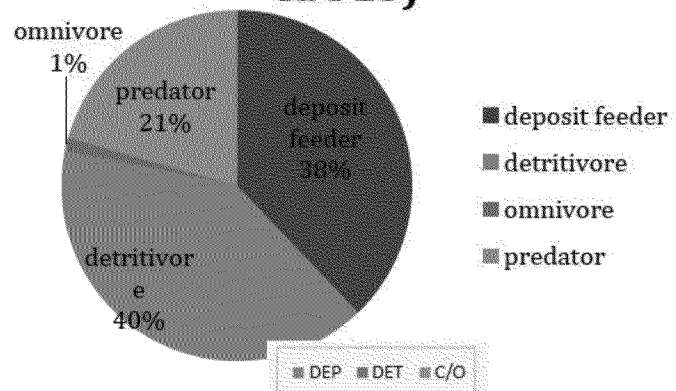
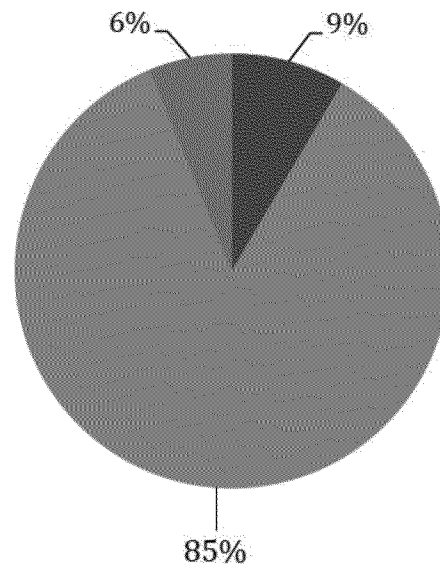


Figure 1 in Appendix E



Changing the RM basis in this manner demonstrates how much the benthic makeup can vary spatially, and that the assumption of a study area-wide detritivore-dominated bed is not supported.

Ultimately, any process of estimating biomass based on organism weights from another site is flawed, especially as the CPG has argued that benthic organisms in the LPR exhibit unique phenotypic differences.

The variability of organism weights across even a single site requires that sample-specific data be used to measure biomass rather than using abundances and estimated mean weights. For example, in the Chesapeake Bay study alone, *Corbicula* weights ranged from 0.00005 grams to 28.26 grams. Extensive variability is also present for all other benthic invertebrates in the Chesapeake Bay study (the average difference between minimum and maximum biomasses when $n > 20$ is over two orders of magnitude, with the mean coefficient of variation being 151%). This variability in the (non-site-specific) data used to estimate biomasses makes the CPG's determination that the LPRSA is dominated by detritivores significantly overstated.

		<p>Finally, as illustrated in the taxa-specific pie chart above, the data used to derive Figure 1 of Appendix E suggest that the Asian clam dominates the detritivore category in terms of biomass. However, based on an examination of the literature used to convert ash-free dry weight (AFDW) to wet weight (Ricciardi and Bourget 1998), the wet weight biomass includes the shells of these clams¹, which should not be considered a viable part of prey biomass. This undoubtedly also inflates the weights of <i>Corbicula</i> relative to other portions of the food web.</p> <p>Ultimately, each of these lines of evidence suggests that the CPG is significantly overstating the quantity of detritivores in the LPR and their role as prey items in the food web. The CPG must discard the flawed analysis that was used to define the proportion of benthic biomass across the LPRSA and a new analysis must be performed. As part of this new analysis, <u>shell-free</u> <i>Corbicula</i> wet weights must be estimated. If non-site-specific biomass data are used, sampling from more than one external site must be included. The CPG must also consider differences in benthic makeup by RM and derive a detritivore-to-deposit-feeder ratio for each salinity zone included in the BERA. Feeding preferences must be modified based on this new analysis (refer to Comment Nos. 615 through 617). Finally, if study area-specific biomass or wet weight data are unavailable, the uncertainty in the benthic invertebrate composition must be thoroughly assessed throughout model calibration and assessment of remedial alternatives.</p>
615	Appendix H, Section 1, page 5, Table 2	<p>The small forage fish diet is assumed to be “65% benthic invertebrates (consumed proportionally to LPRSA biomass).” As discussed in Comment No. 614, the analysis performed to determine the proportion of LPRSA biomass for benthic invertebrates is fundamentally flawed as it combines Chesapeake Bay organism sizes with LPRSA abundance data. Therefore, the proportion of deposit feeders is likely much larger than the CPG’s analysis suggests.</p>

¹ AFDW from the Chesapeake Bay study is converted to wet weight using Ricciardi and Bourget (1998). However, that study states that “Wet weights of molluscs and echinoderms include their shells because they are organically connected.”

		Please modify the small forage fish dietary preference for benthic invertebrates to reflect this fact. The updated dietary preferences should reflect the new analysis described in Comment No. 614 . The uncertainty associated with this analysis should also be evaluated by testing alternative calibrations with a range of feeding preferences for benthic invertebrates.
616	Appendix H, Section 1, page 6, Table 2	The blue crab diet is assumed to be “83% benthic invertebrates (consumed proportionally to LPRSA biomass).” Similar to Comment No. 615 , please modify the blue crab dietary preference for benthic invertebrates to include more deposit feeders, reflecting the new analysis described in Comment No. 614 .
617	Appendix H, Section 1, page 6, Table 2	The common carp diet is assumed to be “54% invertebrates (consumed proportional to abundance in the LPRSA).” Similar to Comment No. 616 , please modify the carp dietary preference for benthic invertebrates to include more deposit feeders, reflecting the new analysis described in Comment No. 614 . This should increase contaminant concentrations in carp tissue and may remove the need for an empirical factor (the “particulate ventilation constant”) to fix the carp calibration.
618	Appendix H, Section 1, page 9, Table 2	As shown in Table 2, American eels of the smaller size class are more closely tied to the sediment in terms of dietary preferences (based on the BERA data analysis, small eels were assumed to consume 80% worms). Therefore, it is not appropriate to exclude these organisms from the model, since the two size classes of eels would be expected to respond differently to different remedial alternatives. Please revise the food web model and calibration assessment to incorporate the small size class of eels, as discussed in Comment No. 604 .